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ANALYSIS OF PROCESSES IN PAIRED-ASSOCIATE LEARNING.

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THE OBJECTIVES OF THIS STUDY WERE--(1) TO DEFINE AND ISOLATE THE BASIC PROCESSES INVOLVED IN PAIR-ASSOCIATE LEARNING, (2) TO SPECIFY THE IMPORTANT VARIABLES RELATED TO THESE PROCESSES AND THE CONDITIONS UNDER WHICH EACH BECOMES IMPORTANT, (3) AND TO DEFINE AND ASSESS THE IMPORTANCE OF THE ASSOCIATION FORMATION PROCESS IN PAIRED-ASSOCIATE LEARNING TASKS. THE PROGRAM INVOLVED THREE PHASES. IN PHASE I A PAIRED-ASSOCIATE LEARNING TASK WAS DEVELOPED IN WHICH THE "ASSOCIATION FORMATION" PROCESS HAD BEEN ISOLATED. IN PHASE II THE ROCK PARADIGM AS MODIFIED BY THE INVESTIGATOR WAS USED TO INVESTIGATE THE ROLE OF REPETITION IN LEARNING THE PAIRED-ASSOCIATE TASK OR TASKS WHICH HAD BEEN DEVELOPED IN PHASE I. A LIST CONSISTING OF TWO SETS OF STIMULUS-RESPONSE PAIRS WAS USED. ALL PAIRS OF ONE SET, THE CONTROL SET, WERE RETAINED IN THE LIST OF ALL LEARNING TRIALS. PAIRS OF THE EXPERIMENTAL SET WERE RETAINED IN THE LIST ONLY IF THE CORRECT RESPONSE WAS GIVEN. IN NOT, THEY WERE REPLACED IN THE LIST BY NEW PAIRS WHICH HAD NOT PREVIOUSLY BEEN SEEN. ON THE BASIS OF THE PRESENT RESULTS, THERE IS NO EVIDENCE THAT EITHER PAIRED-ASSOCIATE LEARNING IN GENERAL OR ANY COMPONENT PROCESS THEREOF TAKES PLACE COMPLETELY IN ALL-OR-NONE FASHION IN A SINGLE TRIAL, EITHER TYPICALLY OR NECESSARILY. (JL)

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ANALYSIS OF PROCESSES IN PAIRED - ASSOCIATE LEARNING

Cooperative Research Project No. 730

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INTRODUCTION

Some form of paired-associate learning, wherein each of several stimuli are to be associated with and responded to by a unique one of a large class of possible alternative responses, has been extensively employed in the experimental investigation of a variety of problems in human learning research, and is also characteristic of a number of tasks involved in educational and other practical learning situations (e.g., foreign-language vocabulary, multiplication tables). Its primary function, however, has been as a method or procedure for the investigation of the role of other variables, such as meaningfulness, similarity, or distribution of practice, with respect to some more general learning phenomenon. In this latter capacity, it has become virtually standard practice to treat paired-associate learning as though it were a single unitary learning process, which builds up gradually in strength with increasing practice or number of repetitions. Although it has been suggested on a number of occasions that more than one distinguishable learning process may be involved in the characteristic paired-associate task, and that one or more of these processes may take place by some means other than a continuous incremental change with practice, this fundamental assumption of a unitary paired-associate learning process building up gradually in strength with repetition has been generally accepted throughout the history of such research, and until recently had never been seriously challenged.

Within the past few years, however, both of these basic assumptions about paired-associate learning have been subjected to an increasing theoretical and experimental attack. Although historically there has been little common ground among those primarily concerned with the possible unitary or multi-process nature of paired-associate learning, and with its relationship to practice or repetition, the close interdependence of the two questions is becoming increasingly evident, and has constituted one of the principal features of the present research project. Nevertheless, in view of their independent histories, relevant research concerning the two questions must necessarily be summarized and reviewed separately.

Of most immediate and direct influence to the present project, as well as to human learning research in general, has been a now-classic experiment by Rock (1957). Rock's results indicated that, contrary to the incremental position, paired-associate items were actually learned completely in all-or-none fashion on a single trial, and that repetition served only to provide increasing opportunities for these all-or-none associations to be learned. This conclusion followed from Rock's finding that under a nonrepetition procedure, wherein each incorrect pair is eliminated from the list after each trial and replaced on the next trial by a new pair which the subject (S) had never seen before, so that each pair must be learned completely on the first trial on which it is presented, performance was equivalent to that under the usual repetition condition, in which the same list of pairs is presented on all trials regardless of their correctness or incorrectness on previous trials. In view of the surprising nature of these results, numerous replications ensued almost immediately (e.g., Clark, Lansford, & Dallenbach, 1960; Wogan & Waters, 1959). A still larger number of investigators were more directly concerned with the evaluation of the role of possible artifacts or uncontrolled variables in Rock's design (e.g., Lockhead, 1961; Postman, 1962; Underwood, Rehula, & Keppel, 1962; Williams, 1961), or with the development of different experimental paradigms for the study of the role of repetition in paired-associate learning (e.g., Bower, 1961, 1962; Estes, 1960; Rock & Heimer, 1959). Inasmuch as most of this work was contemporaneous and overlaps extensively with that undertaken in the present project, the results thereof can most meaningfully be discussed after the description of the procedure and results of the present research.

In an early attempt to replicate the essentials of the Rock experiment with more adequate controls for several variables which may have accounted for his results (Battig, 1959a; also described in Battig, 1962b), it became immediately apparent that a meaningful answer to the question of the all-or-none or incremental nature of paired-associate learning would first require a more precise specification and analysis of various component processes involved in paired-associate tasks than had hitherto been attempted. In contrast with Rock's procedure, wherein different groups of Ss performed under the repetition and nonrepetition conditions, this study employed a within-Ss mixed-lists design such that the single list learned by each S was arbitrarily divided into two equal-sized subsets of pairs. The pairs of one subset were presented on all trials, as in Rock's repetition group, whereas pairs of the other nonrepetition subset were eliminated and replaced by new pairs whenever incorrect, as in Rock's nonrepetition group. Using nonsense-syllable pairs carefully selected so as to be maximally homogeneous in rated "ease of learning" according to a prior scaling procedure (Battig, 1959b), every one of 15 Ss made more errors on the nonrepetition subset, in direct contrast with Rock's finding of no difference between the two conditions. At this stage, the possibility was raised that the learning of nonsense-syllable pairs involves a number of distinguishable processes, and that such materials are therefore inadequate as a test of the restricted hypothesis (apparently although not explicitly advanced by Rock) that a component "association-formation" process takes place in all-or-none fashion. To eliminate the nonassociative forms of learning involved with the nonsense-syllable pairs, materials consisting of highly familiar four-letter words as stimuli with two-digit numbers as responses were used in an otherwise exact replication of the within-Ss repetition-nonrepetition comparison just described. With these materials, performance on the repetition and nonrepetition subsets was virtually identical, offering apparent support for the all-or-none thesis as applied to the association-formation process of paired-associate learning. However, unlike the nonsense-syllable pairs, these word-number pairs varied considerably in learning difficulty, so that the differential selection of the easiest pairs permitted under the nonrepetition condition may have accounted for the finding of no difference (Underwood et al., 1962; Williams, 1961). Moreover, the word-number pairs proved to be extremely easy to learn, suggesting that in addition to their relative freedom of nonassociative learning processes, a major portion of the association-formation process normally required in paired-associate learning had also been eliminated, through the use of stimulus- and response-terms which had already been associated together, directly or indirectly, by the Ss used in this study. Consequently, the word-number pairs were also grossly inadequate as a test of the possible all-or-none nature of a component association-formation process in paired-associate learning.

Following the completion of these two experiments, it became apparent that an extensive research effort would be required to adequately identify and distinguish between the component learning processes involved in paired-associate tasks, a necessary prerequisite to the development of a type of material which was free of all nonassociative processes but required a pure and substantial amount of the association-formation process to be tested for its all-or-none or incremental nature. It later developed that a number of other investigators, at about the same time, had become directly concerned with the possible distinction between various component processes in paired-associate learning, but for reasons largely independent of those influential in the present project. This work was largely unpublished at the time the present project began, although much of it became significantly influential during the course of the project, especially the distinction between response-learning and associative phases by Underwood and his collaborators (Underwood, Runquist, & Schulz, 1959; Underwood & Schulz, 1960) and similar work by other investigators (Cook & Kendler, 1956; McGuire, 1961; Newman, 1961).

The original major purposes of the present project were to find out enough about nonassociative component processes of paired-associate learning to be able to identify and estimate the relative importance of each of these, primarily as a necessary prerequisite to the operational specification of an association-formation process, and the development of materials requiring a pure and substantial case of this process, to be employed under the improved within-Ss procedure described above in an effort to determine whether or not associations build up gradually in strength with repetition, or develop in all-or-none fashion on a single trial. A secondary purpose was to investigate the effects of various task, procedural, and individual-difference variables on relative performance under repetition and nonrepetition conditions, as defined by the Rock paradigm. Several unanticipated but highly interesting and potentially significant results of preliminary research, concerned with the specification of nonassociative component processes in paired-associate learning, significantly changed the course of the project from its original plan, so that a major portion of the project research was directed toward the further investigation of these nonassociative processes, and had little or no direct bearing on the originally primary issue.

Consequently, the description of the experiments in the following section of the report has been divided into three major subsections, respectively presenting the procedure and results of studies primarily concerned with (I) The development of materials and procedures adequate to the investigation of the all-or-none vs. incremental issue in the association-formation process of paired-associate learning; (II) Using the Rock paradigm or modifications thereof, the investigation of the role of repetition in paired-associate learning and its component association-formation process; (III) The specification and analysis of the role of other nonassociative processes in paired-associate learning. Since several of the actual experiments were simultaneously concerned with issues related to more than one of these general problem areas, the relevant aspects of each experiment will be discussed where appropriate in terms of the overall organization, instead of attempting to describe each individual experiment separately. Many of these experiments have been or will be published individually in journal-article form, references to which have been included where appropriate throughout the following section. A separate list of papers presented orally at professional meetings, overlapping extensively with the published references, has been included in the Appendix.

DESCRIPTION OF EXPERIMENTS

I. DEVELOPMENT OF MATERIALS AND PROCEDURES

A. Development of Learning Materials.

As indicated in the introduction, learning materials were needed which (a) required a substantial amount of the association-formation process to be tested for its possible all-or-none nature, but (b) were free of the influence of other component processes known to be of importance in paired-associate learning. The former criterion clearly points to the use of stimulus-terms representing an entirely different class or population of materials from that of the response terms, so that the stimulus- and response-terms are minimally associated or related to each other. The criterion of "purity" of the association-formation process proved to be far more difficult to satisfy, due to the paucity of information concerning the nature and importance of other potentially contaminating processes in paired-associate learning tasks, and to the failure of preliminary experimental attempts to operationally specify such nonassociative processes. Such evidence as was available, however, pointed most directly to learning of the response-terms per se (Underwood & Schulz, 1960) and stimulus discrimination (Gibson, 1940) as the most fundamental of these nonassociative processes. Although the evidence was far less clear-cut, it also appeared that actual stimulus-learning (Underwood & Schulz, 1960) and response-term discrimination (Battig, Williams, & Williams, 1962; Underwood, Runquist, & Schulz, 1959) were of relative unimportance in paired-associate tasks. Consequently, an attempt was made to develop materials wherein the response-terms were highly familiar and well-learned (with their discriminability a lesser but still important consideration), paired with stimulus-terms of high discriminability and low familiarity from an entirely different class of materials.

After consideration of a variety of possibilities, nonsense-shapes or forms as stimuli and two-digit numbers as responses were selected as best meeting the specified requirements. Using a method for the production of random nonsense shapes described by Attneave and Arnoult (1956), a set of 105 shapes were initially constructed representing 7 each of all possible combinations of 5 levels of complexity (4, 6, 8, 12, and 16 sides) with 3 degrees of curvature (0%, 50%, or 100% curved rather than straight sides). To further insure that these shapes were maximally discriminable from each other, (a) no point appeared as a terminus in more than one shape of a given level of complexity; (b) those seven shapes of a given complexity to be included in the same curvature condition were selected so as to be as subjectively dissimilar as possible; and (c) the location, direction, and amount of curvature of the curved sides of shapes within the 50% or 100% curvature conditions were determined so as to be maximally different from other shapes of the set.

In order to provide an empirical check on the discriminability of these shapes, and to ascertain their meaningfulness or association value, the entire set of shapes was scaled for these characteristics by three groups totalling 122 introductory psychology students in a classroom situation. Each of the 105 shapes had been photographed individually and was presented by a 2 x 2 automatic slide projector for 8-sec. while the Ss recorded on a prepared data sheet (a) the name or description of anything the shape reminded them of; and (b) the degree of difficulty (on a numerical five-point rating scale) of making an association to that shape. Immediately following the individual presentation of each shape, a second slide was presented (also for 8 seconds) containing that same shape along with three other shapes from the same complexity-curvature condition, with Ss being instructed to indicate which of the four shapes had been presented on the immediately preceding slide by marking on the prepared data sheet the one of four quadrants of a square which corresponded to the location of that shape on the second slide. To minimize sequential effects, a different unsystematic order of presentation of the 105 shapes was used for each of the three groups of Ss.*

The scaling results indicated (a) that the shapes were in fact highly discriminable since recognition accuracy exceeded 91% for each shape and averaged 97.3% for all shapes combined; and (b) considerable variability among the various shapes as to association value measured either in terms of percentage of Ss giving associations, inter-S consistency of content of these associations, or rated difficulty of making associations, all of which proved to be highly intercorrelated indices. Those shapes, 33 in number, which elicited frequent or consistent associations, or were confused with other shapes of the set by more than one or two Ss, were eliminated. The remaining 72 shapes were then judged for similarity to single-digit numbers by 10 additional Ss, on the basis of which a two-digit number between 12 and 98 (excluding double-numbers and numbers ending in zero) was paired with each shape so as to be minimally similar to it. Independent ratings of all two-digit numbers on a five-point scale for association value were obtained from a new group of 28 Ss, and all shape-number pairs were rated for "ease of learning" on a nine-point scale by still another group of 66 Ss. Finally, measures of actual paired-associate learning difficulty of each shape-number pair were obtained from the pretraining phase of a major experiment involving 80 Ss, described in Section II-C of this report (see also Battig & Brackett, 1961; Brackett & Battig, in press).

Since the results of the extensive pretesting of these materials yielded no evidence of their inadequacy for the purposes for which they were to be used, and suggested no obvious ways in which they could be substantially improved, they were exclusively used in those phases of the overall project where a pure and substantial case of the association-formation process was necessary. In many of the experiments, however, particularly those concerned with the assessment of other nonassociative processes, such materials were either inappropriate or unnecessary. Consequently, extensive use was also made of nonsense-syllable pairs of minimal intrapair similarity and medium association-value previously scaled for learning difficulty (Battig, 1959a, 1960), supplemented by pairs constructed from other calibrated nonsense-syllable materials (Archer, 1960). Limited use has also been made of pairs of semantically similar adjectives (Hilgard, 1951), and of pairs consisting of high association value CVC trigrams or words as stimuli (Archer, 1960), and two-letter (bigram) response-terms of minimal frequency of occurrence in the English language (Underwood & Schulz, 1960).

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* Other details of the scaling procedure and results, not directly relevant to the purposes of the present project, have been published elsewhere (Battig, 1962a).

B. Comparison of Recall and Anticipation Methods in Paired-Associate Learning (Battig & Brackett, 1961).

A possible basic procedural question was raised by the fact that the sequence of presentation of materials for learning and performance-measurement necessitated by the conditions of the original Rock (1957) experiment differ markedly from the anticipation method, which has been sufficiently predominant throughout the history of paired-associate learning research to have approached the status of a standard defining property of paired-associate procedure itself. Under this anticipation method, each stimulus-term is first presented alone while S attempts to pronounce the associated response, and is followed immediately by the presentation of the correct response-term, usually paired with its stimulus. Contrasting with this is the recall method used by Rock, wherein each trial begins with the presentation of the entire list of stimulus-response pairs for learning, at the conclusion of which each of the stimulus-terms is presented alone in a different order for attempted production or recall of the correct response. Such a procedure was necessary for Rock's nonrepetition group in order to permit the replacement of incorrect pairs prior to their presentation again for possible learning, and to avoid the possible elimination of learned pairs which would have been correct on the next trial. Moreover, such a recall procedure, which has been occasionally employed in paired-associate studies for many years, would appear to have the distinct general advantage of allowing for the temporal separation of presentation of materials for learning from measurement of performance, which are necessarily completely confounded under the anticipation method. In addition to its virtually standard usage, arguments offered in favor of the anticipation method include its close correspondence with the serial-anticipation procedure characteristic of serial-learning studies, and its provision of immediate and complete knowledge of results (sometimes termed "reinforcement") following each attempted response, in correspondence with long-standing principles of maximally efficient learning.

Although there was little reason to expect substantial differences between the learning processes involved under these two types of paired-associate procedures, the necessary extensive involvement of the recall method in the present project raised the possibility that the results might be somewhat limited in their applicability to conditions employing the anticipation method, particularly since no direct comparison of the two procedures had been reported in the literature. Consequently, when the opportunity arose to make such a comparison during the pretesting of the shape-number pairs (described in the preceding section) without interfering with the other primary purposes of the experiment of which this constituted a pretraining phase (see Section II-C), this was done and ultimately proved to be a major determinant of the direction of much of the research that followed.

The design of the major experiment required that each of 40 Ss learn two different lists of 12 shape-number pairs in succession on the same day, to a criterion of one errorless trial. The 72 shape-number pairs (see p. 6) had been divided into six sets of 12 so that the shapes of each set were approximately equal in average association value and the number responses covered the entire range of two-digit numbers. Each of the six sets was learned as a list by an approximately equal number of Ss, as was each possible pair of lists, but no significant differences in performance between lists were found (F less than 1). Each S learned one of his two lists under each method, half using the anticipation method for the first list and the recall method for the second, whereas the reverse order (recall-anticipation) was used for the other 20 Ss. A "trial" under both methods consisted of 24 item presentations (the 12 shape-number (S-R) pairs and the 12 shapes (S) alone) for 5-sec. each by automatic slide projector, with S being instructed to repeat aloud each number during its presentation and to try to give the correct number when each shape was presented alone. The methods differed only in the sequence of S-R and S-term presentations within a trial. Under the anticipation method, the S-terms and S-R pairs appeared alternately, each S-term being followed immediately by the corresponding S-R pair, the complete sequence of events within a trial being as follows (S_1; S_1-R_1; S_2; S_2-R_2; S_3; S_3-R_3; .... S_{12}; S_{12}-R_{12}). Under the recall method, each trial began with the presentation of all 12 S-R pairs, after which the 12 S-terms were presented alone in a different order, represented as follows (S_1-R_1; S_2- R_2; S_3-R_3; .... S_{12}-R_{12}; S_3; S_{12}; S_1; .... S_2). To make the procedures more exactly comparable, the first anticipation trial was preceded by the presentation of the 12 S-R pairs exactly as in the first half of each recall trial. A different unsystematic order of presentation was used each time through the list, and a 30-sec. interval was introduced between trials and between the presentation of S-R pairs and S-terms alone within each recall trial, to allow E to rearrange the order of the slides.*

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* This added 30-sec. interval within the recall-method trial represents an uncontrolled difference between the two methods. However, intertrial intervals of this order of magnitude are known to have little or no effect on paired-associate learning (Underwood, 1961b). Nevertheless, this added interval was eliminated in all subsequent experiments in which the two methods were compared.

The results of this experiment are presented in Fig. 1 and Table 1, both of which clearly show a substantial and consistent superiority of performance under the recall method, which was demonstrated by analysis of variance to be highly significant both for total errors (*F* = 25.68, df = 1/38) and for trials to criterion (*F* = 26.74). A significant overall improvement in performance with practice was also obtained for both errors (*F* = 18.10) and trials (*F* = 10.30), but the attenuation of the differences between methods on the second as compared with the first list was not significant in either case (*Fs* = 1.33 & 1.03). Since first-trial performance did not differ for the two methods (*F* = 1.36), initial equivalence can be assumed and the inferiority of the anticipation method cannot be attributed to the shift in procedure between the initial presentation of S-R pairs and the first actual anticipation trial.

The obtained differences in favor of the recall method were not only completely unexpected, but run directly counter to long-established principles concerning the necessary conditions for optimally efficient learning. In particular, since no information or knowledge of results of any kind was given during S-term presentation under the recall method (making it comparable to the anticipation method in this respect), delay of knowledge of results until the pairs were presented during the next trial becomes a major distinguishing feature of the recall method, particularly as compared with the immediate and complete knowledge of results characteristic of the anticipation method. As a result, the present results directly contradict the principle that immediate knowledge of results is a necessary condition for efficient learning. This principle, along with the desirability of breaking up the material to be learned into very small response units which also characterizes the anticipation method, have represented basic assumptions underlying the recent development of programmed learning techniques (e.g., Skinner, 1958). In view of the close comparability between the anticipation method and the sequences of events involved in the typical teaching-machine program, the present finding of superior performance under the recall method strongly implies that currently accepted programming principles do not represent a universally optimum procedure for learning efficiency.

Although directly relevant to the present project only in their suggestion that the results of studies employing the recall method may not be generalizable to paired-associate learning under the anticipation procedure, these findings are of obvious significance with respect to more general theoretical and practical problems, and therefore seemed to call for further investigation under this project. As a consequence, the recall-anticipation variable was subsequently incorporated into two major multi-factor experiments using nonsense-syllable materials, which are described in detail in Section III. The complete results of these studies are presented in Tables 12 and 15, from which it is clear that neither showed differences between recall and anticipation methods of the magnitude obtained in the first experiment. However, a numerical although statistically insignificant overall superiority of the recall

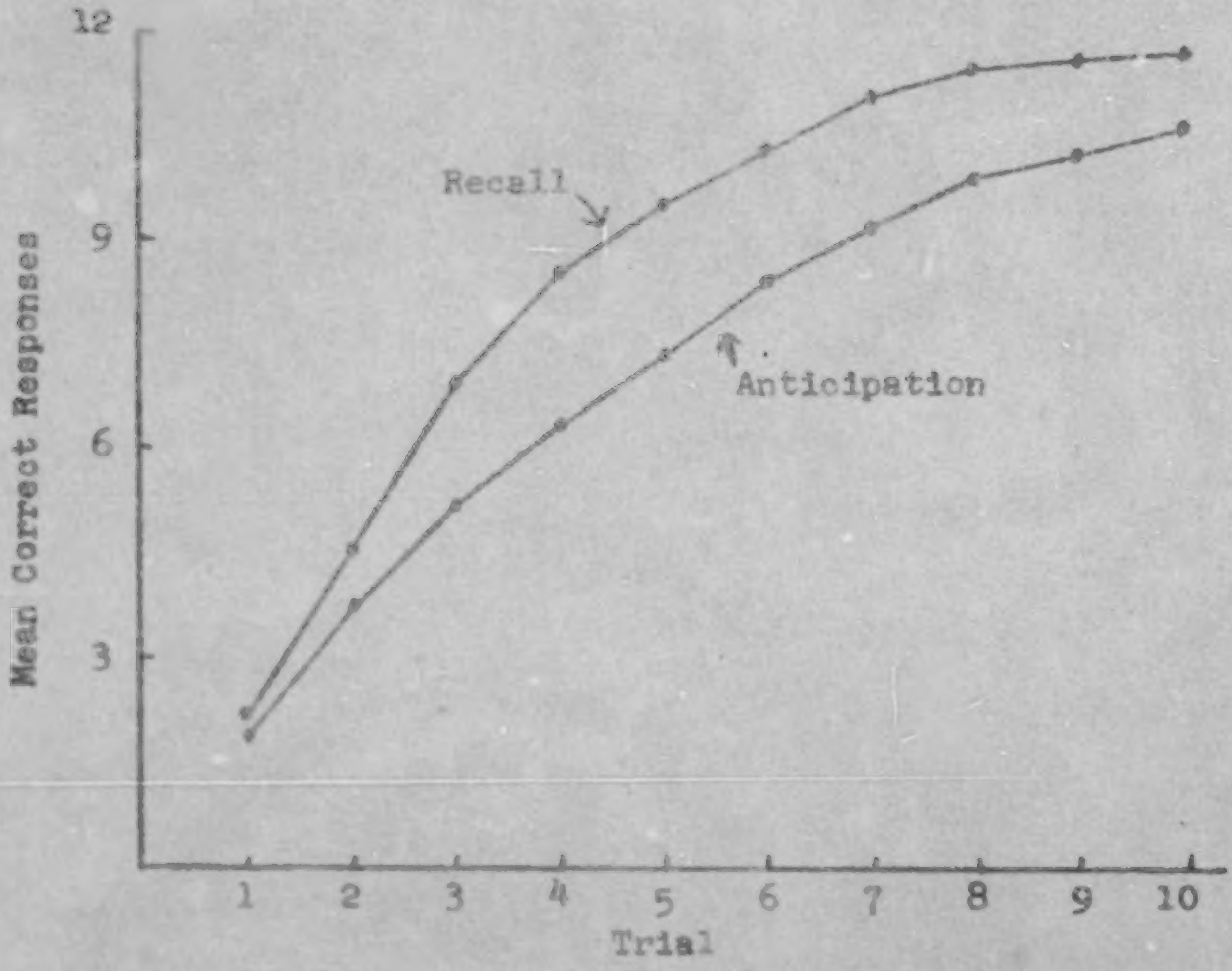

Fig. 1 - Mean correct responses per trial under Anticipation and Recall methods.

TABLE 1

Mean Trials and Total Errors for Each Method and List

	Anticipation Method			Recall Method		
	List 1	List 2	Total	List 1	List 2	Total
Errors	60.75	38.40	49.58	35.55	28.10	31.83
Trials	11.15	8.35	9.75	7.25	6.45	6.85

method was obtained under a variety of conditions comparable to the first study except for their use of nonsense-syllable materials. Only when the list was presented in a constant serial order on all trials, or when 50% of the response-terms were omitted on each trial, was better performance obtained under the anticipation method and in neither case was this superiority statistically significant. Contrary to an early hypothesis, there was no evidence that intralist similarity was significantly related to the recall-anticipation method differences. Although the relevant variables remain to be determined, it is apparent that the superiority of the recall to the anticipation method may be limited to certain kinds of materials and conditions, and that somehow the initial study with its shape-number pairs had proved to be optimal in this respect. Research on this problem continues following the termination of the present project, with current emphasis on the possibility that the recall method may be particularly advantageous for learning involving the stimulus-term, and that its superiority with the shape-number pairs is attributable to the probable relative importance of stimulus-learning with these materials. Should this hypothesis receive empirical support, further indication would be provided that different processes, or at least differential emphases on various processes, may be involved in paired-associate learning under the anticipation and recall methods. It is already clear from research completed under this project that the effects of other variables (e.g., constant vs. varied serial-order, percent of response-terms omitted) are different under the two methods, so that this previously ignored procedural variable will require extensive attention in future paired-associate learning research.

C. Knowledge of Results in Paired-Associate Learning (Brackett & Battig, in press).

Still another unexpected finding concerning a procedural variable, which is remarkably consistent with some implications of the recall-anticipation method comparisons, emerged from the first large-scale experiment of this project. An examination of possible procedural differences which might account for inconsistencies in results between several early studies under the Rock paradigm suggested the presence or absence of knowledge of results (right or wrong) following each attempted response as a likely possibility. To empirically test this hypothesis, two groups of 40 Ss each learned 12-item paired-associate nonsense-syllable lists under the recall method with or without knowledge of results (right-wrong information) following each attempted response, the two groups being further subdivided into repetition and nonrepetition groups corresponding to those employed by Rock (1957). Further details of procedure and results relevant to the latter comparison are presented in Section II-C. The result of primary interest here concerns the virtual equivalence of performance under the knowledge-of-results and no-knowledge-of-results conditions, both under repetition and nonrepetition conditions, as is shown in Table 7. The failure of knowledge of results to facilitate learning was sufficiently surprising that this variable was subsequently incorporated in another experiment (see Section II-C for full description), with virtually identical results. The only evidence that the knowledge-of-results variable had any effect whatsoever came from an analysis of the changes in number of correct responses over trials for the two experiments combined, which revealed a significantly greater overall slope of the combined knowledge-of-results learning curves than for the no-knowledge-of-results conditions ($\underline{F} = 8.34$, df = 1/148, $\underline{P}$ less than .01). However, neither the slight no-knowledge-of-results superiority on early trials, nor the somewhat greater knowledge-of-results superiority on the final trials, was statistically significant, and the groups also did not differ significantly in trials to criterion.

Once again the major implications of this finding are extraneous to the primary concerns of the present project. However, taken in combination with the results of the recall-anticipation method comparison, impressive support is provided for the more general contention that immediate knowledge of results is not a critical factor in paired-associate learning, and that this variable as defined in human-learning situations cannot meaningfully be equated to "reinforcement" in simpler animal-learning studies, with which it has been frequently compared and confused. The lack of effect of the knowledge-of-results variable in these studies was directly responsible for the elimination of any immediate right-wrong information in all subsequent project experiments using the recall method, a procedure which would appear to have general desirability inasmuch as it eliminates the principal outlet by which learning can occur during the performance-measurement (recall) phase of the trial, and serves to enhance the separation between learning and performance-measurement inherent in the recall method itself.

D. General Description of Procedure.

Although there was considerable variation in procedural details from one experiment to another, the subsequent discussion of the several major experiments will be facilitated if certain of these which were common to most of the experiments or which are of relative unimportance with respect to the purposes and interpretations thereof are first described separately. With the exception of studies in which the variable was intentionally manipulated, and the experiments described in Section III-A, the recall method was employed throughout the project. Similarly, with one exception where the variable was directly manipulated (Section II-C), standard-type paired-associate learning instructions were used, which thoroughly explained and illustrated the basic aspects of task and procedure, and encouraged S (in some instances by added financial reward) to learn the material as completely and rapidly as possible, but gave little information as to the explicit purpose of the experiment. Most of the Ss were regular or summer-session college students at the University of Virginia who were paid for their services at the rate of $1 for each experimental session (all of which were planned to last an hour or less). Some students, however, were obtained from introductory psychology classes wherein service as experimental Ss fulfilled a course requirement. Due to the limited population of available Ss during much of the project, application of refined sampling procedures or selective criteria was infeasible, although Ss who were uncooperative or unusual in their performance in one experiment were as a matter of course dropped from the pool of Ss to be contacted for future experiments. Extensive efforts were made, however, to insure that the assignment of Ss to groups or conditions within a single experiment was completely unbiased, this being based typically on a predetermined random or unsystematic sequence in terms of order of appearance in the laboratory. Within-Ss designs, wherein each S served simultaneously or successively in two or more experimental conditions, were adopted in all instances where they could be used effectively without contaminating the variable(s) of primary interest. In some instances, it was also possible to assign Ss to conditions or groups on the basis of a previous (pretraining or first-trial) performance measure, or to statistically adjust final performance measures on the basis of early differences in performance, so as to increase the precision in the subsequent statistical analysis. It should be made clear, however, that generalization of the present results beyond that portion of a college-student population interested in (or willing to be talked into) participation in verbal learning experiments must proceed with considerable caution.

Partly due to the policy of the Cooperative Research Branch prohibiting the use of project funds for the purchase of capital equipment, several pieces of apparatus and equipment, obtained (often on a temporary basis) from a variety of sources, were employed in the various experiments for the presentation of materials for learning and performance measurement. Because of its advantages for controlled yet flexible use as a presentation device, automatic 2 x 2 slide projectors were employed wherever possible. Other exposure devices included a standard mechanical memory drum, Dunning animatic strip-film projector, and simple manual presentation of 3 x 5 in. cards. In addition to availability, selection of apparatus depended heavily on the demands of the experimental conditions with respect to such factors as presentation rate and the feasibility of arranging a fixed sequence of item presentations prior to and independent of performance during the experiment. The memory-drum and strip-film projector were thus restricted to experiments involving a predetermined fixed item-sequence and rapid (2-sec.) presentation rates with short (4-8 sec.) intertrial intervals, as contrasted with the slower (4-5 sec.) presentation rates and longer (typically 30-sec.) intertrial intervals employed in all studies using an automatic slide projector, and the still slower (7-sec.) rate adopted for manual presentation of cards. Lists in all cases were 10-12 items in length, and learning was carried out to a performance criterion of one or two errorless trials, although in all instances wherein a substantial proportion of Ss were unable to reach the specified performance criterion within the time allotted to the experimental session, the experiment was terminated after a previously specified maximum number (10-12) of trials. Although the resulting dual criterion presented certain analytic complications, it appeared the only practical way to avoid the even more serious problems created by selective elimination of slow learners, and permitted the inclusion of comparable performance measures for all Ss regardless of individual differences in learning ability.

II. STUDIES OF THE ROLE OF REPETITION IN PAIRED-ASSOCIATE LEARNING.

In line with the primary purposes of the project, as described in the original research proposal and in the introduction to this report, several experiments were designed to investigate various facets of the question of the role of repetition in paired-associate learning, and the extent to which such learning occurs in complete all-or-none fashion within a single trial as opposed to a gradual incremental buildup in strength with repeated trials. Although differing considerably from the actual order in which the experiments were conducted, a more organized and meaningful description of this phase of the project can be provided by describing the experiments on the basis of their overall significance with respect to the underlying theoretical issues involved. More specifically, the first two experiments to be described represented direct attempts to determine the role of repetition in the association-formation phase or process of paired-associate learning, to be followed by three subsections describing experiments primarily concerned with the effects of several variables inherent in or related to the Rock paradigm on the results obtained when the repetition and nonrepetition conditions are compared.

A. Learning of shape-number pairs under simultaneous repetition and nonrepetition conditions (Battig, 1962b).

As soon as the extensive process of developing and calibrating the shape-number pairs had been completed, these were used in a replication of the within-Ss modification of the Rock paradigm employed in the two preliminary studies described in the Introduction. For purposes of this study, the 72 shape-number pairs were divided into 12 subsets of 6 each which were simultaneously matched as closely as possible for association value of both shapes and numbers, and for actual and rated learning difficulty of the shape-number pairs (based on the scaling results described in Section I-A). An initial list consisting of two of these equivalent six-pair subsets was formed for each of 15 Ss, the repetition subset remaining intact on all trials, whereas in the nonrepetition subset, all pairs not responded to correctly on a given trial were removed from the list and replaced on the next trial by new pairs taken from another of the equivalent subsets of pairs. Each subset was used equally often for the various Ss as the repetition and initial nonrepetition subset, and at the various points in the sequence of nonrepetition replacement pairs. Learning was carried out to a criterion of two successive errorless trials, following which each S was given a recognition test for incorrect replaced nonrepetition pairs, consisting of the presentation of 10 of these pairs intermixed with 10 additional pairs from the pool of 72 which S had never seen, with S indicating in each case whether or not he thought he had seen the pair previously during the experiment. After the recognition test, each S was shown again each of the 12 pairs in the list he had finally learned, and was asked to describe verbally how he had learned them.

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TABLE 2

Repetition and Nonrepetition Performance Measures

	Trials	Total Errors	Omissions	Other Errors
Repetition	5.80	16.00	1.93	14.07
Nonrepetition	6.65	20.07	4.13	15.93

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As shown in Table 2 above and Fig. 2 on the next page, performance was consistently better on pairs of the repetition subset, significantly so for total errors (t = 2.83, P less than .02) although not for trials (t = 1.85, P between .05 and .10). Further examination revealed that all but 4 of the 15 Ss made fewer total errors on repetition pairs, and that these differences were primarily attributable to the greater number of response omissions for pairs of the nonrepetition subset (t = 3.06, P less than .01), whereas other errors (intrusions and partially correct responses) did not differ significantly under the two conditions (t = 1.48). Further substantiating the conclusion that something had been learned about the incorrect nonrepetition pairs, and that their removal impeded overall learning of the list, were the results of the recognition test, which revealed 71.7% correct identifications, significantly above the 50% chance level (t = 9.12). Mediated bases for associating the shape and number together were verbalized for only 24% of the nonrepetition pairs which were learned, and 20% of the repetition pairs, indicating that the shape-number materials had been reasonably successful in requiring a substantial amount of association-formation, and in minimizing the possibility of selective elimination of difficult pairs from the nonrepetition subset.

Although the differences in favor of repetition performance achieved statistical significance for the most appropriate error measures, they were relatively small in absolute magnitude, which could be attributed at least in part to the fact that only 12 pairs were learned by each S and most of these were responded to correctly for the first time within two or three trials, thereby rendering the procedure quite insensitive for the detection of the effects of repetition on initial learning. In an effort to provide a more convincing demonstration that these materials are not learned in a single trial in all-or-none fashion, another experiment was run under a modified procedure, as described below.

B. Role of Repetition in Paired-Associate Learning with All Correct (Learned) Pairs Eliminated (Battig, 1962b).

The principal modification in the procedure for this experiment consisted of the elimination of all correct pairs, both from the repetition and nonrepetition subsets, after each trial. As a result, all pairs of the nonrepetition subset were presented only for a single trial, and were replaced by a previously unseen but otherwise equivalent subset after each trial. All

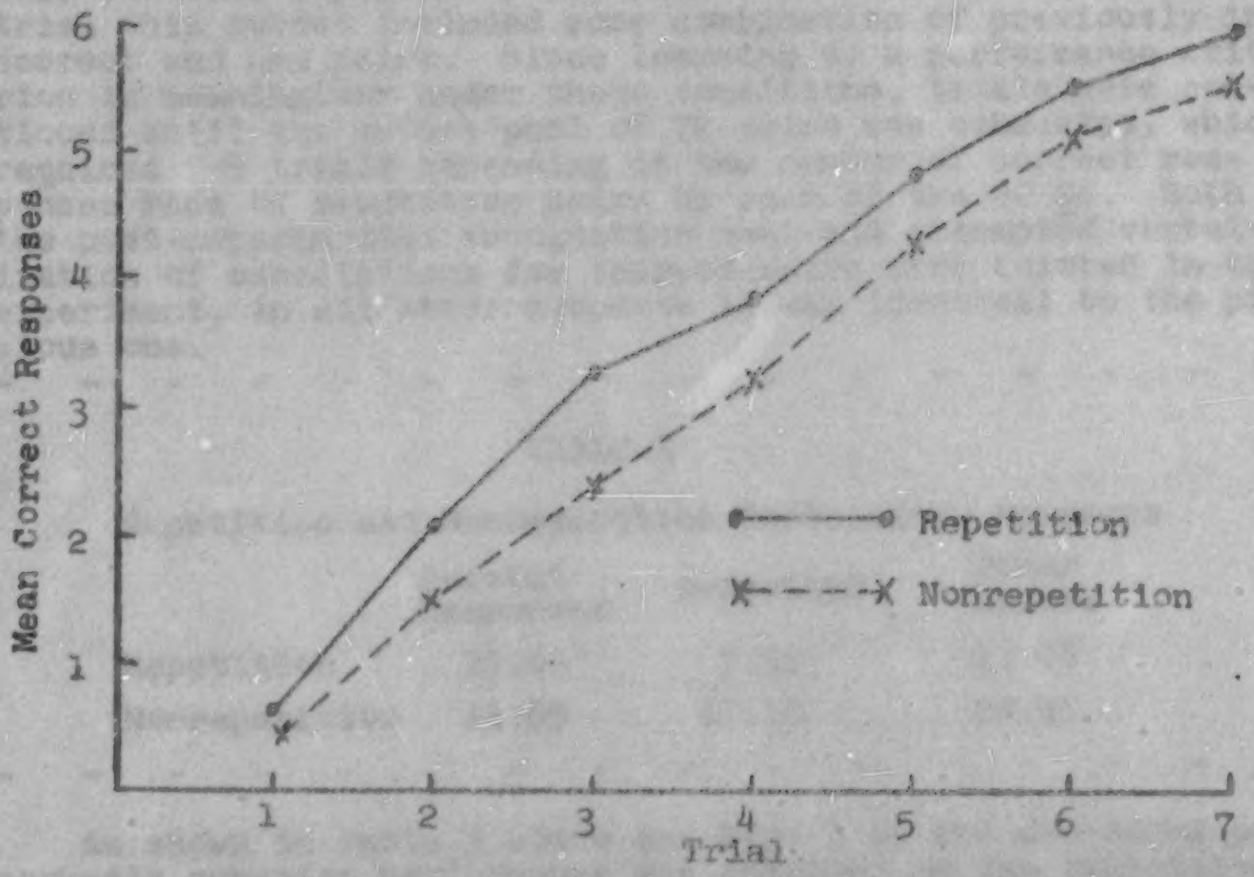

Fig. 2 - Mean correct responses per trial for repetition and nonrepetition conditions.

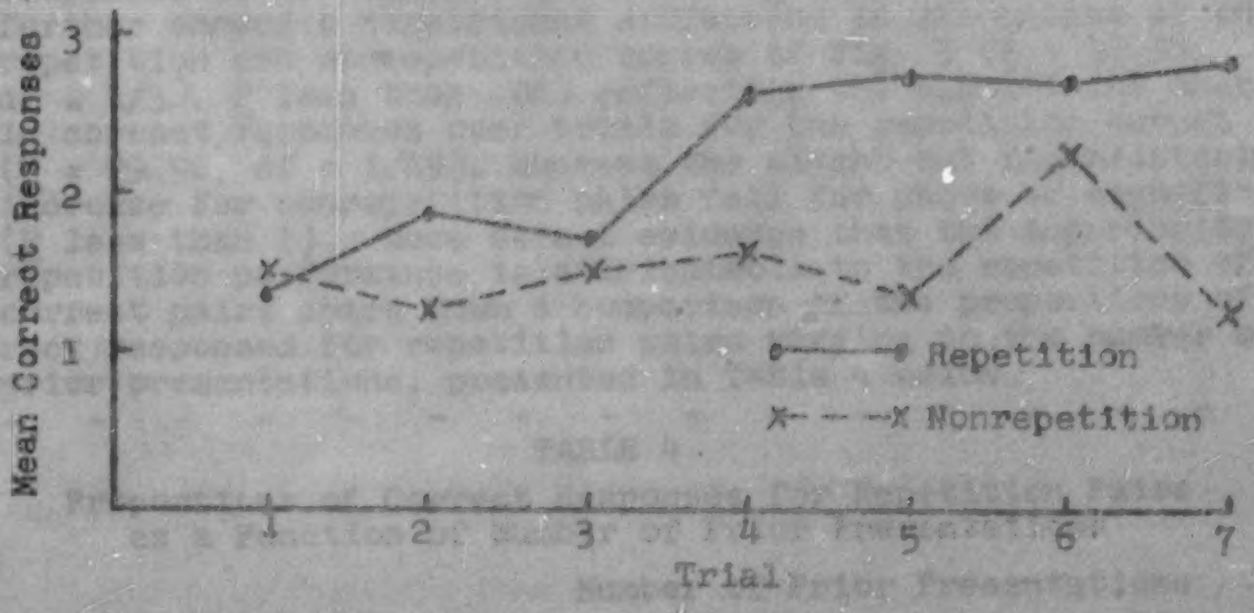

Fig. 3 - Mean correct responses per trial for repetition and nonrepetition conditions with all correct pairs removed after each trial.

incorrect pairs of the repetition subset were retained in the list until responded to correctly for the first time whereupon they too were replaced by new pairs, so that on any particular trial this subset included some combination of previously incorrect and new pairs. Since learning to a performance criterion is meaningless under these conditions, trials were continued until the entire pool of 72 pairs was exhausted, which required 7-9 trials depending on the number of correct responses made to repetition pairs by each of the 20 *Ss*. Both the post-experimental recognition test and attempted verbalization of associations for learned pairs were omitted in this experiment, in all other respects it was identical to the previous one.

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TABLE 3

Repetition and Nonrepetition Performance Measures

	Correct Responses	Omissions	Other Errors
Repetition	17,85	7.50	21.75
Nonrepetition	11.65	11.10	24.35

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As shown in Table 3 above and Fig. 3 on the preceding page, markedly superior performance was obtained on the repetition subset on all trials after the first. Of the 20 *Ss*, 18 made more total correct responses to the repetition subset, and the other two performed identically on the two subsets. Trend analysis of variance revealed the difference in total correct responses to be highly significant (*F* = 49.42, df = 1/19) and further showed a significant difference in the slopes of the repetition and nonrepetition curves of Fig. 3 (*F* = 10.35, df = 1/38, *P* less than .01) reflecting the significant increase in correct responses over trials for the repetition subset (*F* = 29.92, df = 1/19), whereas the slight but inconsistent increase for nonrepetition pairs fell far short of significance (*F* less than 1). More direct evidence that the superiority of repetition performance is attributable to the repetition of incorrect pairs comes from a comparison of the proportions of correct responses for repetition pairs varying in the number of prior presentations, presented in Table 4 below.

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TABLE 4

Proportions of Correct Responses for Repetition Pairs as a Function of Number of Prior Presentations

	Number of Prior Presentations				
	One	Two	Three	Four	Five & Over
Total Number	420	265	135	66	56
Correct Responses	118	114	61	32	28
Total Proportion	.281	.430	.452	.485	.500
Mean Proportion	.270	.424	.496	.581	.539

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In addition to total number of pairs presented, correct responses, and proportion, Table 4 also gives the mean proportions of correct responses averaged over Ss, a measure relatively freer of the bias resulting from the disproportionate contribution of slow learners to the total proportion measures, especially for more frequently presented pairs. By either index, proportion of correct responses is seen to increase consistently with number of prior presentations. Trend analysis of variance of the proportions of first, second, and third presentations responded to correctly by each S showed this increase to be highly significant ($F = 24.56$, df = 1/19). These results offer incontrovertible evidence against the all-or-none thesis as applied to the learning of the present shape-number pairs. To the extent that these materials are accepted as a satisfactory operational specification of the association-formation process, they also speak strongly against the restricted form of this hypothesis as applied to a component process of paired-associate learning.

TABLE 5

Proportions of Correct Responses after Varying Numbers of Prior Presentations

	Number of Prior Presentations							
	1	2	3	4	5	6	7	8-plus
Total Number	650	483	336	230	156	102	69	109
Correct Rs	113	115	84	56	37	27	18	27
Total Prop.	.174	.238	.250	.244	.237	.265	.261	.248

Presented in Table 5 are results similar to those of Table 4 from another experiment wherein the procedure for the entire list of nonsense-syllable pairs corresponded to that for the repetition subset of the previous study, in that all correct pairs were removed from the list after each trial and replaced by new pairs, whereas all incorrect pairs were retained in the list. All pairs used in this experiment were constructed from syllables rated individually for association value by each of 24 Ss several days prior to the experiment. The initial list for each S thus consisted of an equal number of each of the four possible combinations of stimulus- and response-terms which had been rated high (H) and low (L) in association value by that particular S, with each replacement pair representing the same association-value condition as the pair which it had replaced. Although somewhat less consistent than the results of the previous study, a considerable increase in proportion of correct responses with increasing number of prior presentations is evident in the data presented in Table 5. Additional empirical support is thus provided for the conclusions drawn from the results of eliminating all correct pairs after each trial in the earlier study, although with materials and conditions

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* As in the previous experiment, significantly more omission errors were made on nonrepetition pairs ($t = 2.86$, P less than .01) but the difference shown in Table 4 in other errors was not significant ($t = 1.58$).

which differed considerably for the two studies.*

C. Effect of Method of Pretraining and Knowledge of Results on Repetition and Nonrepetition Performance (Brackett & Battig, in press).

Although this research was conducted sequentially in the form of two separate experiments, these were virtually identical to each other in all respects except for the manipulation of the method of pretraining variable, so that they were analyzed and can most efficiently be described together. Their principal purpose was to investigate two variables inherent in the Rock paradigm as possible alternatives to one-trial all-or-none learning as a means of accounting for Rock's finding of no difference between repetition and nonrepetition performance: (A) Method of learning. Rock's nonrepetition procedure is such as to encourage S to concentrate on learning a few pairs in the list on each trial, while ignoring the remainder. To the extent that S adopts such a more efficient "part" learning strategy, whereby he ignores and consequently learns nothing about the eliminated incorrect nonrepetition pairs, the finding that nonrepetition performance is not retarded as compared with the repetition group could be accounted for solely on the basis of the different method of learning employed by these Ss. Should such a "part" strategy also be more efficient for paired-associate learning in general, the case for differences in method of learning, as a means of accounting for the apparent one-trial learning purportedly demonstrated under the Rock paradigm, would be further strengthened. Unfortunately, neither experimental control nor direct information have been provided as to the extent to which Ss of either group concentrated differentially on learning a few pairs on each trial in any of the studies within the Rock paradigm. However, the fact that Rock explicitly instructed his Ss to "try to learn all pairs on each trial" (Rock, 1957, p. 187) suggests that his repetition Ss may not have adopted a part strategy, but that the obvious appropriateness of such a strategy nevertheless may have led to its increasing use by his nonrepetition group, further indicating the likelihood of differences between repetition and nonrepetition conditions in method of learning. (B) *Knowledge of Results*. The possible relevance of this variable was noted at the very beginning of the project, at which time the only two known studies which showed significant differences in favor of the repetition over the nonrepetition group within the Rock paradigm were also the only ones which had *not* given knowledge of results, in the form of "right" or "wrong" following each attempted response (Chenzoff & Gregg, 1959; Reed & Riach, 1960). This raised the possibility that the continued presentation of pairs to which the experimenter (*E*) had said "wrong" for the repetition group may have represented a disturbing factor as regards subsequent performance on these pairs, which was not present for the nonrepetition group since all "wrong" pairs were thereupon eliminated. Since the elimination of knowledge of results would minimize differences between the groups in this respect, greater differences in favor of repetition performance might thus be expected under no-knowledge-of-results conditions.

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* This study (which was supported only in part under the present project) has been reported in full elsewhere (Kepros, Bourne, & Battig, 1962). Of considerable general interest was the additional finding that, although all nonsense syllables had been selected from a narrow range of association-values on the basis of group scaling procedures, significant differences were found as a function of individually-determined high or low association value, indicating the limited applicability of averaged group scaling results to the individual S. Contrary to previous studies, these effects were limited to the association value of the stimulus-term, a discrepancy which may be attributable to the use of the recall method and/or the modified repetition procedure, either of which may have enhanced the importance of stimulus learning and consequently the effects of meaningfulness of the stimulus-term, as compared with the usual conditions under which this variable has been investigated.

The two experiments combined can be regarded as a complete 2 x 2 x 3 factorial design where one of 12 groups represented each possible combination of all values of the following three variables: (A) Repetition and nonrepetition conditions identical to those employed by Rock (1957); (B) Presence or absence of knowledge of results (right or wrong) after each attempted response; (C) Whole, part, or no pretraining as described below, with all *Ss* in the first experiment receiving no pretraining whereas *Ss* in the second experiment received either whole or part pretraining, as diagrammed in Table 6.

TABLE 6 - Design of Experiment

	Repetition		Nonrepetition	
Knowledge of Results	Present	Absent	Present	Absent
Experiment I No Pretraining	40	40	40	40
Experiment II Whole Pretraining	20	20	20	20
Part Pretraining	20	20	20	20

(Numbers (20 or 40) represent number of *Ss* in each group)

All *Ss* given whole pretraining learned on the preceding day two lists of shape-number pairs, each to a criterion of one errorless trial (the procedure and results of which have been described in Section I-B). The instructions to these *Ss*, both before and after the pretraining lists, included several statements indicating that the entire list was to be learned and that this could be done most efficiently by trying to learn all pairs on each trial. In contrast, each *S* given part pretraining learned six lists of shape-number pairs (three each under recall and anticipation methods in counterbalanced order) to a criterion of 4 out of 12 correct responses, and was instructed both before and after pretraining to try to concentrate on learning a few pairs on each trial, since this would be the most efficient method of learning. On the day following pretraining (or on the first day in the first experiment where no pretraining was given) each *S* learned, to a criterion of one errorless trial or a maximum of 12 trials under either repetition or nonrepetition conditions, a list of 12 nonsense-syllable pairs representing one of six equal subsets of pairs as to scaled learning difficulty (Battig, 1959b), each subset being used equally often as the repetition and initial nonrepetition subset and at the various points in the sequence of nonrepetition replacement pairs.

TABLE 7

Mean Total Repetition and Nonrepetition Errors for Pretraining and Knowledge-of-Results Conditions

Pretraining Condition	Repetition			Nonrepetition			Total
	K of R Present	K of R Absent	Total	K of R Present	K of R Absent	Total	
No	33.20	31.70	32.45	59.10	63.55	61.33	46.89
Whole	34.30	38.30	36.30	58.00	66.40	62.20	49.25
Part	33.00	33.30	33.15	35.10	35.80	35.45	34.30
Total	33.43	33.75	33.59	52.83	57.33	55.08	

Mean total errors for each of the 12 groups and for each value of the three independent variables are presented in Table 7 above. The knowledge-of-results variable, the absence of overall effects of which have already been discussed in Section I-C, did not interact with either of the other variables (all *F*s less than 1 by analysis of variance). The fact that relative performance under repetition and nonrepetition conditions was virtually identical with and without knowledge of results offers no indication that differences with respect to this variable have any relevance to the discrepant results obtained by various investigators within the Rock paradigm, or that knowledge of results has any effect whatsoever on performance under either repetition or nonrepetition conditions.

However, Table 7 reveals sizable overall differences in total errors between repetition and nonrepetition groups for both no and whole pretraining conditions, whereas the part-pretraining nonrepetition group performed at a level indistinguishable from any of the repetition groups, which were almost identical within all three pretraining conditions. These effects were substantiated by analysis of variance, which showed significant overall differences between pretraining (*F* = 4.58, df = 2/148, *P* less than .025) and repetition-nonrepetition conditions (*F* = 30.71, df = 1/148, *P* less than .01), as well as a significant interaction between these two variables (*F* = 4.13, df = 2/148, *P* less than .025). Also analyzed were changes in performance in the various groups over trial blocks 1-4, 5-8, and 9-12, which with the exception of the knowledge-of-results variable (see p. 12) showed all differences between groups to be consistent and significant on all three trial blocks. However, an increasing superiority of the repetition over the nonrepetition condition on later trials was indicated by a significant interaction of this variable with trial blocks ($F = 25.63$, df - 2/296, $\underline{P}$ less than .01).*

These results demonstrate conclusively that relative performance under repetition and nonrepetition conditions depends heavily on the method of learning used during pretraining, and presumably also adopted in learning the list following pretraining. They indicate that if Ss adopt a part learning method, nonrepetition performance is equivalent to that of the repetition group, whereas the repetition condition is significantly superior when Ss attempt to learn all pairs in the list on each trial, as emphasized under the whole pretraining conditions. The close equivalence of both repetition and nonrepetition performance for the whole and no pretraining conditions suggests that the nonrepetition Ss given no pretraining in this study had probably not shifted significantly to a part method, as further indicated by the significant superiority of the repetition over the nonrepetition group under no pretraining conditions. However, the obvious suggestion that nonrepetition Ss do adopt some form of part method in those studies where they perform as well as the repetition group runs into apparent complications, since the no pretraining groups of the present study were not explicitly instructed to "try to learn all pairs on each trial" as Rock (1957, p. 187) had done. In this connection, it should be noted that minor instructional variations of this kind are notorious for their inconsistent and unreliable effects on verbal learning tasks. Consequently, a differential adoption of a part learning method by the nonrepetition group remains a plausible means of at least partially accounting for the discrepancies in results obtained under the Rock paradigm. Since the meaningful use of this paradigm with respect to the underlying theoretical issue concerning the gradual or all-or-none nature of paired-associate learning is clearly contingent upon equal distribution of learning effort over all pairs of the list, the present results emphasize the necessity of instituting more effective and precise controls for this factor in future studies pertaining to the all-or-none issue.

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* Similar differences between groups were evident in the trials-to-criterion measure, although the failure of a majority (51%) of Ss to attain the criterion of one errorless trial prevented a meaningful analysis of these data. In view of the sizable individual differences between Ss in performance in this experiment, extensive efforts were made to analyze these differences and to relate them to performance under the various experimental conditions. However, separate analyses of Ss above and below median performance in the various groups revealed no evidence of differences as a function of individual performance level, which also proved to be insignificantly related to scores on the College Board Examinations ($\underline{r} = .18$). The data from Ss with high and low overall performance measures were also examined and analyzed separately in several other project experiments. However, in all cases where this appeared to interact with differences between conditions, the interaction could be attributed to the generally high or low difficulty of the task (i.e., if the task was easy, differences between conditions were greater for low-performing Ss; if difficult, high Ss showed greater differences). Since these individual difference effects were thus not consistent in direction and contributed little to the understanding or interpretation of the results of this or any of the other experiments, they will be omitted from further presentation.

D. Effect of Kind of Material and Previous Experience on Repetition and Nonrepetition Performance (Battig & Nelson, 1962).

This study originated in part as an extension of the line of research described in the preceding subsection, to determine whether experience in learning several lists under repetition and nonrepetition conditions led to significant differences in subsequent performance under either condition, or to the differential adoption of part and whole methods of learning. In order to minimize specific interlist transfer effects, a wide variety of learning materials of different kinds and levels of difficulty were employed in this study, which was designed so as to provide for systematic investigation of this variable as related to performance under repetition and nonrepetition conditions.

Each of 24 Ss learned, on each of four successive days, a different one of four lists of paired-associate materials. Two lists consisted of the nonsense-syllable and shape-number pairs employed in the studies previously described in this section, while the other two lists were constructed from the adjective pairs and word-bigram pairs described in Section I-A. The shape-number pairs represented a test-list, learned as the fourth and last list by all Ss. The order in which the other three (pretraining) lists were learned was completely counterbalanced, so that each of the six possible sequences was used for four Ss, one in each of the four major groups. Half of the Ss learned all three pretraining lists under the repetition condition, the other half under the nonrepetition condition. The repetition and nonrepetition pretraining groups were further subdivided in terms of test-list conditions, half of each group using the repetition method, the other half the nonrepetition method.

Mean total errors to criterion are presented in Table 8 at the top of the next page for each of the four subgroups and for the combined repetition and nonrepetition groups, separately for each of the four lists. In addition to the obvious and highly significant ($F = 58.22$) differences between lists, analysis of variance revealed the overall superiority of repetition over nonrepetition performance on pretraining lists to be significant ($F = 6.49$, df = 1/15, P less than .025). A significant repetition-nonrepetition by pretraining lists interaction ($F = 5.93$, df = 2/32, P less than .01) was shown by subsequent analysis to be attributable to the absence of any superiority of the repetition condition for word-bigram pairs (F less than 1), as contrasted with a significant difference in favor of repetition performance for both nonsense-syllable (F = 7.18) and adjective (F = 7.70) pairs. In agreement with the results for these materials under the within-Ss design (Section II-A), the test-list of shape-number pairs showed significantly fewer errors for the repetition than the nonrepetition groups (F = 7.55, df = 1/21, P less than .025). However, test-list performance was unaffected in any way by whether the pretraining lists were learned under repetition or nonrepetition conditions (F less than 1), offering no indication of any differences in learning method developed by nonrepetition as compared with repetition pretraining. Since most Ss had previously served in other paired-associate learning experiments, and all of the lists (except the nonsense-syllable pairs) were sufficiently easy to learn that little difference actually existed in practice between the repetition and nonrepetition procedures, these findings of no effect of pretraining may reflect an inadequate or insensitive manipulation of differential previous experience as a variable, rather than demonstrating that the variable itself is ineffective.

TABLE 8

Mean Total Errors under Repetition (R) and Nonrepetition (NR) Conditions for Each Group and Type of List

	Pretraining Lists			Test List
Group	Nonsense-Syllable	Word-Bigram	Adjective	Shape-Number
R-R	(R) 44.0	(R) 29.2	(R) 6.2	(R) 27.8
R-NR	(R) 34.3	(R) 22.3	(R) 3.7	(NR) 45.0
NR-R	(NR) 67.5	(NR) 27.7	(NR) 9.3	(R) 22.7
NR-NR	(NR) 69.5	(NR) 28.3	(NR)11.0	(NR) 46.0
Condition				
R	39.2	25.8	4.9	25.3
NR	68.5	28.0	10.2	45.5

The finding of no difference between repetition and nonrepetition performance on the word-bigram pairs can probably be attributed to the fact that these pairs had not been previously equated in terms of scaled learning difficulty, as had all of the other materials. That they in fact were decidedly heterogeneous in difficulty was shown by a high positive correlation (r = .502) between (a) percentage of correct responses to each word-bigram pair by the repetition groups; and (b) percentage of nonrepetition Ss responding correctly to each pair on its first presentation. The fact that the superiority of repetition to nonrepetition performance was of closely comparable magnitude for each of the other sets of materials, all of which were known to be of greater homogeneity than the word-bigram pairs, suggests that heterogeneity of difficulty within the set of materials, rather than the overall level of difficulty of the entire set of materials, represents the major source of any effects of kind of material on repetition and nonrepetition performance.

E. Differences in Interpair Interference under Repetition and Nonrepetition Conditions (Brown, 1961).

Another possibly important difference between repetition and nonrepetition conditions within the Rock paradigm concerns the different contexts of other pairs within which any given pair must be learned. More specifically, each unlearned repetition pair, in addition to being presented itself on repeated occasions, must be learned as part of a list which also includes a combination of previously presented learned (correct) and other incorrect pairs. Each pair for the nonrepetition group, on the other hand, is always presented in a context from which previously incorrect pairs have been excluded, which consists entirely of correct and new previously unseen pairs. If previously repeated unlearned pairs produce more interference with the learning of other pairs in the list than do new pairs presented for the first time, performance of the nonrepetition group would clearly be facilitated thereby, suggesting still another alternative besides one-trial all-or-none learning as a means of accounting for the failures of the repetition group to perform better than the nonrepetition group within the Rock paradigm.*

The present experiment was designed to directly determine the relative amounts of interpair interference produced by (a) previously presented _learned_ (correct) pairs; (b) previously presented _unlearned_ (incorrect) pairs; and (c) previously unseen _new_ pairs; on performance on other pairs in a paired-associate list. Each of 80 _Ss_ first learned the same list of nonsense-syllable pairs to a criterion of 6 out of 12 correct responses, following which they were divided into four matched groups of 20 each differing in the composition of the list subsequently presented to them. The _learned-unlearned_ group continued to practice on exactly the same list as before, whereas six new previously unseen pairs were substituted for the six unlearned pairs and presented along with the six learned pairs for the _learned-new_ group. For the _unlearned-new_ group, on the other hand, the revised list consisted of the same six new pairs (replacing the learned pairs) along with the six unlearned pairs. Finally, a fourth _new-new_ group learned a list consisting of the six new pairs used for the previous two groups, along with six additional new pairs.

Table 9 on the next page presents the mean number of errors subsequently made on previously learned, unlearned, and new pairs within each of the four groups. There is little evidence here of differences in the amount of interpair interference

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* A similar hypothesis has been suggested independently, and some indirect evidence offered in its support, by Clark, Lansford, and Dallenbach (1960) and by Kristofferson (1961).

TABLE 9

Mean Total Errors on Three Types of Pairs for Each Group

Type of Pairs	Group Learned-Unlearned	Learned-New	Unlearned-New	New-New
Learned	6.0	4.3	-	-
Unlearned	12-9	-	17.9	-
New	-	13.5	16.0	17.7

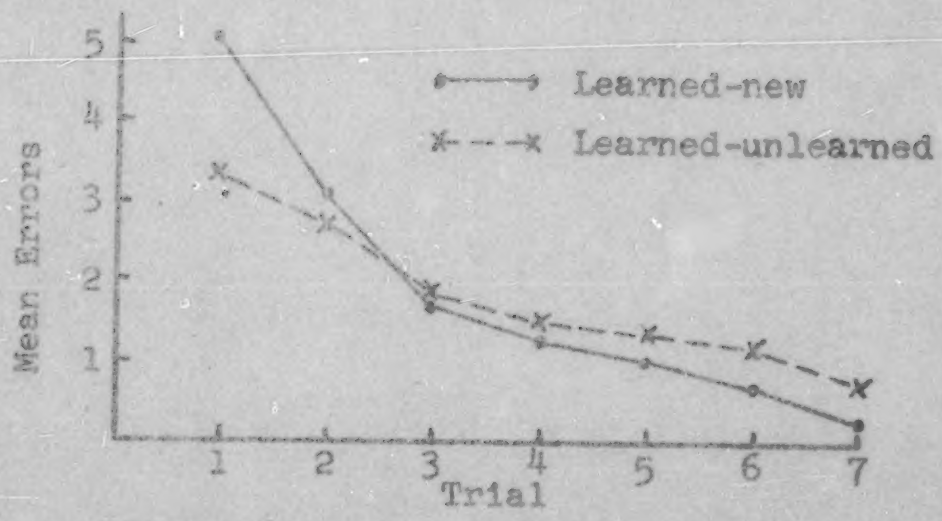

Fig. 4 - Mean errors per trial on unlearned and new pairs for learned-unlearned and learned-new groups.

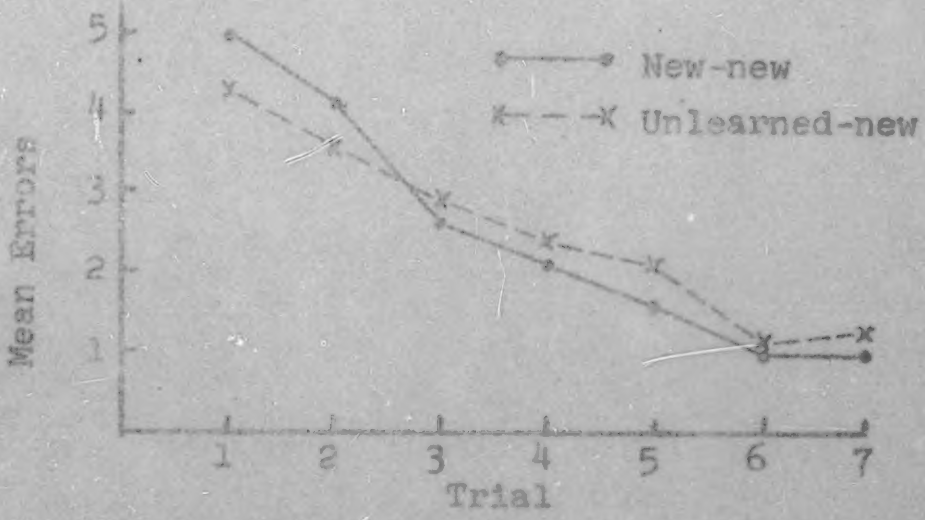

Fig. 5 - Mean errors per trial on unlearned and new pairs for unlearned-new and new-new groups.

produced by unlearned and new pairs, since neither the difference in favor of the learned-new group over the learned-unlearned group in performance on learned pairs, nor the opposite difference in new-pair performance favoring the unlearned-new over the new-new group, approached significance by analysis of covariance (both Fs less than 1). However, all relevant comparisons reveal superior performance on both unlearned and new pairs when learned in the context of learned pairs, as compared with a context of either unlearned or new pairs. These differences were significant in the case of unlearned-pair performance between the learned-unlearned and unlearned-new groups (F = 7.72, df = 1/37, P less than .01), and in new-pair performance between the learned-new and new-new groups (F = 4.64, P less than .05), but the difference between the learned-new and unlearned-new groups in new-pair performance fell short of significance (F = 2.34).

Also of considerable interest with respect to the issue of all-or-none learning are the findings revealed in Fig. 4 and 5 on the preceding page, presenting comparative performance measures over trials on unlearned and new pairs when learned in a context of learned pairs (Fig. 4) and with new pairs (Fig. 5). Both comparisons display fewer errors for the first two trials on unlearned pairs, shifting to superior performance on new pairs on later trials. Trend analysis of variance revealed the differences in slopes between the unlearned- and new-pair curves to be significant, both for the learned-unlearned and learned-new groups in Fig. 4 (F = 15.86, df = 1/38, P less than .01) and for the unlearned-new and new-new groups in Fig. 5 (F = 4.94, P less than .05). This finding of poorer performance on late trials on unlearned pairs suggests that these were generally more difficult to learn than the new pairs. Consequently, the fact that fewer errors were made on the first two trials on unlearned pairs, despite their greater difficulty, strongly implies that these pairs had been partially learned even though not responded to correctly as part of the initial list, in direct conflict with the all-or-none position. This evidence of substantial learning on repeated unlearned pairs also offers a possible explanation for the failure of these pairs to produce more interpair interference than did new pairs, on the grounds that under the present conditions the unlearned pairs had been presented sufficiently often that they more closely resembled learned than new pairs, and were beyond the stage of learning at which maximal interpair interference would be produced. This suggestion, along with the limited sensitivity of the present procedure for the detection of differential interpair interference effects, renders the above results inconclusive with respect to the original question, and has led to further efforts to develop procedures adequate for the experimental determination of the effect of stage of learning on interpair interference effects, of which the following exploratory investigation was carried out as part of the present project.

In this experiment, each of 36 Ss practiced on different nonoverlapping lists of paired-associate items under each of three conditions of interpair interference. Two of these conditions, differing only in number of nonsense-syllable pairs presented (9 or 12) involved a single presentation of each pair, followed by a single presentation of each stimulus-term alone for attempted recall, the procedure corresponding to a typical single recall-method trial. Under the third condition, the entire list of 12 pairs was presented five times prior to the single stimulus-term presentations. Immediately following these preliminary conditions, a second list consisting of six pairs of common three-letter words (Archer, 1960), along with six of the previously presented nonsense-syllable pairs which had not been responded to correctly, was presented for a single recall-method trial, followed immediately by a second presentation of the stimulus-terms alone for attempted recall, and then by a previously prepared recognition test. Order of presentation of the three conditions, and the particular lists of pairs used within each condition, were both completely counterbalanced across Ss with respect to order of presentation and to each other.

Since high intercorrelations were obtained between performance measures on both recall trials and the recognition test, and relative performance under the three conditions was similar for each measure, the results are presented and were analyzed in terms of a combined error score on the six word-pairs summed over all three measures. Performance according to this combined measure differed only slightly under the three conditions, mean total errors ranging from 9.4 for the 9-pair list presented once, to 8.7 and 8.5 for the 12-pair list presented five times and once, respectively. This difference failed to approach significance by analysis of variance (F less than 1), wherein only the partially confounded conditions by order of presentation interaction proved to be significant ($F = 3.82$, $df = 6/60$, P less than .01), suggesting that differential transfer effects from preceding conditions may have obscured any differences between conditions with respect to interfering effects. Moreover, differences between conditions based only on the first list learned by each S, although still insignificant (F less than 1), proved to be diametrically opposed in direction to the results combined over all three lists, since the smallest mean number of errors (9.7) was made on the 9-pair list presented once, whereas 10.2 and 11.6 mean errors were made on the 12-pair list presented five times and once, respectively. These latter differences, uncontaminated by differential transfer effects from previous lists, are more consistent with the expectation that maximal interpair interference should be produced by pairs from longer lists presented a relatively small number of times. An alternative explanation for the failure to find differential interfering effects may be that the common word-pairs were too different from the nonsense-syllable pairs to be substantially interfered with by them. The use of these materials had been originally suggested by the finding in the previous experiment that too little is learned about nonsense-syllable pairs on a single trial to be sensitive indices, but the present results suggest that some other way must be found to solve this problem of sensitivity of measurement. Research on this problem is continuing, but currently remains in the procedural development stage, and has as yet been unsuccessful in demonstrating differences in interpair interference as a function of frequency of presentation of incorrect pairs.

III. STUDIES OF OTHER PROCESSES IN PAIRED-ASSOCIATE LEARNING.

In this final section, two lines of experimentation will be described which are for the most part entirely unrelated to the experiments described in the previous two sections, and have in common little beyond their mutual concern with the identification and evaluation of processes other than the association-formation process known or hypothesized to be involved in paired-associate tasks. Further complicating the description of these experiments is the fact that, largely as a result of unanticipated or inconsistent findings in the preliminary stages of this research, it became necessary to incorporate a number of other procedural and task variables into complex multiple-factor designs in more recent investigations concerned with these problems, which were primarily responsible for and have largely occupied the third-year extension of the original two-year contract. The first series of experiments to be described grew out of an attempt during the first year of the project to develop a method for operationally distinguishing the association-formation process of paired-associate learning from discrimination and familiarization processes, but ultimately became primarily concerned with the relationship (or lack of same) between processes involved in verbal-discrimination and paired-associate learning. The second, still in progress following the termination of the present project, concerns the role played by serial-position cues, resulting from the necessary presentation of items in some temporal order or sequence, in the paired-associate learning of these materials.

A. Transfer from Verbal-Discrimination to Paired-Associate Learning (Battig, Williams, & Williams, 1962).

Three separate but closely related major experiments have been completed in which paired-associate learning was preceded by the verbal-discrimination learning of lists of items bearing several different relationships to the items of the subsequent paired-associate list. The verbal-discrimination learning procedure, wherein a list of pairs of items are presented just as in the typical paired-associate list but Ss task is merely to learn to respond by pronouncing the one member of each pair arbitrarily designated as "correct," was viewed as a means of producing separately the familiarization and discrimination processes involved in paired-associate learning, and of evaluating the relative amount and importance of these processes as compared with the association-formation process which is not required in the verbal-dsicrimination task.

In the first experiment, each of 36 Ss learned exactly the same paired-associate list of 12 nonsense-syllable pairs under standard memory-drum anticipation conditions to a criterion of two successive errorless trials or a maximum of 24 trials, after first having learned a verbal-discrimination list composed in part of the same nonsense-syllables as in the paired-associate list. As illustrated in the left-half of Table 10 on the next page, three basic relationships existed between the verbal-discrimination and paired-associate pairs, with four of the 12

TABLE 10

Design of Verbal-Discrimination (VD) Transfer Experiments

First Experiment VD List R	W*	Condition	Paired-Associate List (Both Experiments) S.	R#	Condition	Second Experiment VD List R	W
BIY	ROJ	D(S)	BIY	ROJ	D(S)	BIY	ROJ
JAF	LIH	D(S)	JAF	LIH	D(S)	JAF	LIH
KUX	VOR	D(R)	VOR	KUX	D(R)	KUX	VOR
PIB	QQAW	D(R)	QAW	PIB	D(R)	PIB	QAW
RUZ	ciq	F(S)	RUZ	TEY	L(S-R)	RUZ	ZOG
yun	TEY	F(S)	ZOG	DEQ	L	TEY	VAK
ZOG	qer	F(R)	PEH	VAK	L(S)	PEH	SUQ
gah	DEQ	F(R)	SUQ	GED	L(R)	GED	DEQ
bov	PEH	N	TIV	JEC	U(S-R)	TIV	QUM
VAK	zes	N	NUK	ZIL	U	JEC	YIS
jiz	SUQ	N	WOX	QUM	U(S)	WOX	ZIL
GED	jub	N	YIS	MUP	U(R)	MUP	NUK

* R = right; W = wrong. # S = Stimulus; R = Response.

(Note: VD syllables not used in Paired-Associate list written in lower-case letters).

TABLE 11

Mean Correct Paired-Associate Responses (First Two Experiments)

First Experiment	Type of Pair D-pairs	F-pairs	N-pairs
(S) pairs	10.61	7.25	-
(R) pairs	8.82	8.08	-
Total	9.72	7.67	9.88

Second Experiment		D-pairs	L and U pairs	Control Group
	(S)	11.22	(L) 9.26	-
	(R)	10.63	(U) 9.33	-
Total		10.92	9.29	10.57

paired-associate pairs each representing (a) *Discriminated* (*D*) pairs which were identical for the two tasks; (b) *Familiarized* (*F*) pairs which consisted of syllables included in the verbal-discrimination list, but which had been paired therein with other syllables not included in the paired-associate list; and (c) *New* (*N*) pairs which consisted entirely of syllables not used in the verbal-discrimination list. The D and F pairs were further subdivided into (S) and (R) subsets of two each, the (S) pairs consisting of stimulus-terms which had been "right" and the response-terms "wrong" on the verbal-discrimination task, whereas the reverse was true for the paired-associate pairs of the (R) subsets. To control for possible differences in pair difficulty, a total of six different verbal-discrimination lists were used, so that each paired-associate pair represented the D, F, and N subsets as well as the (S) and (R) subsets for the same number (six) of *Ss*.

It was expected that paired-associate performance would be best on the D-pairs and poorest on the N-pairs, and that the extent of a discrimination process would be reflected by the difference in favor of the D-pairs (which had been both discriminated and familiarized during verbal-discrimination learning) over the F-pairs (which had only been familiarized). Similarly, the assumed difference in favor of the F-pairs over the N (unfamiliarized) pairs would represent the magnitude of the familiarization process. To minimize the contamination of these measures by general sources of intertask transfer, the verbal-discrimination procedure was made to differ from that for paired-associate learning in that items were presented on 3 x 5 in. cards under self-paced conditions. In addition, the left-right positions of the syllables of each verbal-discrimination pair were reversed on half the trials, the serial order of the list was different on each trial, and learning was carried out to a criterion of two successive errorless trials. Immediately after reaching this criterion, the "wrong" syllables of all pairs were presented individually, and *S* was asked to try to recall the "right" syllable paired with it, as a check on the extent to which intrapair associations had been formed during verbal-discrimination learning.

As shown in Table 11 on the preceding page, the expected positive transfer effects to paired-associate learning of the D- and F-pairs were not obtained. Instead, the largest number of correct responses were actually made on pairs of the N subset, and analysis of variance showed performance on the F-pairs to be significantly below that for either the D-pairs (*F* = 11.54, df = 1/150, *P* less than .01) or for the N-pairs (*F* = 13.41), which did not differ significantly from each other (*F* less than 1). Further confounding expectation was the failure to find significant differences in favor of the (R) over the (S) subsets. Instead, the superiority of D(S)- over D(R)-pairs in performance shown in Table 11 proved to be statistically significant (*F* = 8.83, df = 1/150, *P* less than .01), although a significant interaction of the F and D with the (S) and (R) subsets (*F* = 4.74, df = 1/150, *P* less than .05). resulted from the slight contrasting superiority of performance on F(R) over F(S) pairs. However, the combined D(S) and F(S) subsets were significantly superior to the corresponding (R) subsets in number of errors preceding the first correct response to each pair ($\underline{F} = 5.99$, $\underline{P}$ less than .01).* These latter findings are difficult to reconcile with the known greater importance of response- over stimulus-learning in paired-associate tasks (e.g., Underwood & Schulz, 1960), from which it would follow that putting "right" verbal-discrimination syllables, about which more has been learned, into the response-position of the paired-associate pairs, about which most needs to be learned, should maximally facilitate subsequent paired-associate performance. That better performance was found instead on the (S) subset suggests that the processes involved in verbal-discrimination learning are more appropriate to learning of the paired-associate stimulus, and have little relevance to paired-associate response-learning, leading to the further implication that discrimination processes are of little importance with respect to the response-terms of a paired-associate task (see p. 5 of this report).

Since the principal results of this experiment were almost diametrically opposed to expectation, it was decided to perform a second experiment wherein the essentials of the procedure were replicated, but with a few modifications designed to explicate possible means of accounting for the original results. As depicted in the right half of Table 10, exactly the same paired-associate list and procedure was employed, but several changes were introduced in the nature of the relationships between pairings in the verbal-discrimination and paired-associate lists. Once again three basic types of relationships were used, one of which corresponded exactly to the D-pairs of the first experiment. However, the F- and N-pairs of the first experiment were replaced by two types of pairs which consisted entirely of syllables which had been included in the verbal-discrimination list, but had been paired differently therein from the paired-associate pairs. Four of the paired-associate pairs, designated as the like-position (L) condition, consisted of stimulus-terms which had been paired in the verbal-discrimination list with the stimulus-term from another paired-associate pair, and of response-terms which had been paired with another paired-associate response-term in the verbal-discrimination list. The remaining four paired-associate pairs consisted of stimulus-terms which had been paired in the verbal-discrimination list with the response-term of another paired-associate pair, and of response-terms paired with another paired-associate stimulus-term, which have therefore been designated as *unlike-position* (U) pairs.* To check on the possibility that the absence of positive transfer in the first experiment might have been due to the relative novelty of the N-pairs resulting from the use therein of a within-*S*s mixed-lists type of design, a separate control group of 24 *S*s was also run, for whom the verbal-discrimination list consisted exclusively of syllables *not* used in the paired-associate list, and maximally dissimilar from the paired-associate syllables. The only other important difference from the first experiment was the expansion of the recall-test given at the conclusion of verbal-discrimination learning to include the individual presentation of both the "right" and "wrong" syllable from each pair of the verbal-discrimination list.

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* Both this measure and the percentage of errors following the first correct response were determined and analyzed in all experiments reported in this section, but these will be presented and discussed only where they yielded substantially different results from the total correct responses measure (or total errors, its reciprocal). A more complete published report of the first two experiments, including measures of verbal-discrimination performance which are not directly relevant to the present project, can be found elsewhere (Battig, Williams, & Williams, 1962), and a similar complete report of the third experiment is also planned (Battig & Brackett, in preparation).

As shown in Table 11, the results were in all important respects closely comparable to those of the first experiment. Although slightly more average correct responses were given to D-pairs than by the control group, this difference fell far short of significance by analysis of variance (F less than 1). Performance on both L- and U-pairs combined, which differed insignificantly from each other (F less than 1), was significantly below that for D-pairs ($F = 10.37$, $df = 1/264$, P less than .01) or for the control group ($F = 9.76$).# The difference between D(S) and D(R) pairs, although in the same direction as the first experiment, fell far short of significance (F less than 1). It is apparent from these results that the absence of facilitation in performance on D-pairs in the first experiment was not a chance finding and that if the syllables are paired differently in the verbal-discrimination and paired-associate lists, significant negative effects are produced on paired-associate performance. Moreover, these negative transfer effects are no greater when the verbal-discrimination pairings are with syllables from other paired-associate pairs, as in the L and U conditions, than they are with syllables not included in the paired-associate list, as with the F-pairs of the first experiment.

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* As shown in Table 10, both the L- and U-pairs represented all possible combinations of number and position of "right" and "wrong" syllables from the verbal-discrimination list. Since no systematic differences of any kind were found between the four types of pairs within either the L or U subsets, they will be omitted from further consideration in this report.

# These control-group comparisons are based on the within-*S*s error variance from the analysis of variance for the group given D-, L-, and U-pairs, and the differences fell far short of significance (F less than 1) for the more strictly proper between-groups analysis which necessarily includes between-*S*s variability in the error term, the discrepancy being due to the sizable differences between individual *S*s in overall performance level under these conditions. Since the absolute magnitude of the differences involving the control group are closely similar to the corresponding differences based on the N-pairs in the first experiment, and the amount of between-*S*s variability was also similar in both experiments, the present procedure can be defended on the grounds that it provides a more meaningful basis for comparison between corresponding conditions within the two experiments.

This latter finding offers a complication with respect to the most obvious explanation for the observed negative-transfer effects, based on the learning of associations between the syllables within verbal-discrimination pairs which interfered with the subsequent learning of the different associations required in the paired-associate list, since such interfering effects should be greater when the same items are paired differently in the two lists than when different items are used in the pairings of the two lists (e.g., Porter & Duncan, 1953). An even more serious difficulty with such an interpretation, however, comes from the results of the recall-test given immediately after verbal-discrimination learning in both experiments as a check on the extent to which intrapair associations had been learned therein. This recall test revealed correct associations for 17% of the verbal-discrimination pairs in the first experiment, increasing to 21% on the expanded recall test used in the second experiment. As expected, paired-associate performance proved to be significantly better on correctly associated D-pairs in both experiments combined ($t = 2.35$, p less than .05). However, significantly better paired-associate performance was also obtained on pairs which included one or more correctly associated syllables on the recall test for the combined F, L, and U conditions ($t = 2.78$, p less than .01), despite the fact that these syllables had been paired differently in the verbal-discrimination list, so that new and different associations had to be learned to these syllables during paired-associate learning. In other words, the observed negative transfer effects were primarily due to paired-associate pairs consisting of nonassociated syllables from the verbal-discrimination list, rather than to those for which measurable incorrect associations had developed as a result of verbal-discrimination learning. The consequent implication that the observed negative-transfer effects can be attributed to partial or subthreshold associations developed during verbal-discrimination learning is consistent both with the conclusion that paired-associate learning does not represent an all-or-none process, and with the as yet unconfirmed hypothesis (discussed in Section II-E) that partially-learned pairs produce maximal interference with the learning of other pairs.

The third experiment of this series, carried out on a considerably larger scale than the first two, represented in part an effort to determine the generality of the transfer relationships between verbal-discrimination and paired-associate learning, through the manipulation of three task and procedural variables which might conceivably produce differences in these transfer phenomena, as well as being of interest in their own right. Each of 144 *Ss* learned first a verbal-discrimination list followed by a paired-associate list, the pairs of the two lists being interrelated in exactly the same way as in the first experiment and as represented in the left half of Table 10.* Each of 12 equal-sized groups represented one of the possible combinations of each value of three variables in a complete factorial 3 x 2 x 2 design, as follows:

(A) *Intralist similarity*. One of the three values of this variable, designated as the *low-similarity* condition was represented by a list within which each letter of the alphabet (except Q) was used at least once, but no vowel appeared more than five times, no consonant was used more than three times and never more than once as either the first or last letter in either a stimulus- or response-term. For a second *medium-similarity* list, the stimulus-terms were constructed from eight consonants (C G K M P R S V) used three times each, and four vowels (A E I U), whereas a partially overlapping set of eight consonants (B F G K L N R T) used three times each and four vowels (A E I O) constituted the response-terms. For the *high-similarity* list, only four consonants (K M R S) and three vowels (E I U) composed the stimulus-terms, with the response-terms also constructed from four consonants (B F R L) with A, I, and O as the vowels (except for one E). In constructing each list, extensive efforts were made to hold similarity of stimulus- to response-terms at a low level, both within and between pairs, and to keep the lists for the three similarity conditions approximately equivalent in this respect.

(B) *Recall-anticipation method*. Since the first two experiments had used the anticipation method whereas the recall method had been employed in all other project research, incorporation of these two methods as an additional variable permitted the determination of the applicability of the previous findings concerning the relationships between verbal-discrimination and paired-associate learning to the actual conditions typical of the present project, as well as providing a check on the generality of the results of the original comparison between the two methods described in Section I-B.

(C) *Percentage ORM (occurrence of response-members)*. In part because the previous two experiments had suggested that verbal-discrimination learning affects primarily paired-associate learning of the stimulus-term, the third variable incorporated into this experiment involved a comparison of the usual paired-associate procedure wherein *all* response-terms were presented on each trial, designated as *100% ORM*, with a *50% ORM* condition under which each of the response-terms was presented on only half of the trials and only half of the response-terms were presented on any given trial, based on a completely predetermined but unsystematic schedule. An additional reason for the inclusion of the ORM variable was to test the hypothesis that the previously found superiority of the recall over the anticipation method (Section I-B) was due to the complete separation of presentation of the list of pairs for learning and of stimulus-terms alone for performance (recall) measurement within the recall method, and that the elimination of this clear-cut separation of these two functions under the 50% ORM condition might therefore destroy the superiority of recall over anticipation performance.

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* A minor exception to this statement was the use of each of the four possible combinations of number and position of "right" and "wrong" syllables from verbal-discrimination pairs in the four F-pairs for each *S*, a variable which had no effect on performance (in agreement with the first two experiments) and consequently will not be discussed further.

Except for the use of different lists consisting of nonsense-syllables of higher (75-85%) association value as compared with the 34-37% syllables (Archer, 1960) used in the first two experiments, and the presentation of the paired-associate lists via strip-film projector rather than memory drum, all other aspects of design and procedure were identical to the first experiment.

Table 12 on the next page presents the mean total correct paired-associate responses per pair for the D, F, and N subsets for each of the 12 groups separately, as well as combined for the three major between-group variables. Although there are some discrepancies between the 12 groups, the overall results largely substantiate those of the previous two experiments in that performance was poorest on the F-pairs and slightly superior on D- to N-pairs. Analysis of variance revealed the overall differences between D-, F-, and N-pairs to be significant (*F* = 4.26, df = 2/1479, *P* less than .02). However, this difference was largely attributable to the superiority of D- to F-pairs (*F* = 8.34, *P* less than .01), while neither that of N- over F-pairs (*F* = 3.27, *P* less than .10) nor that of D- to N-pairs (*F* = 1.17) proved to be statistically significant. Although the overall interaction between the recall-anticipation method variable and D-F-N subsets fell short of significance (*F* = 2.25, df = 2/1479, *P* approximately .10), the observed differences between performance on D-, F-, and N-pairs were due almost entirely to groups performing under the anticipation method (F = 6.47, *P* less than .01), whereas performance was virtually identical on the three types of pairs under the recall method (*F* less than 1). None of the other interactions between D-F-N performance and the three between-group variables approached significance, either alone or in combination, and the overall groups by D-F-N interaction fell far short of significance (*F* = 1.18).

Sizable and highly significant differences in overall paired-associate performance were produced by both the similarity (*F* = 48.03) and ORM variables (F = 12.02). In contrast with the results of the experiment described in Section I-B, the overall difference in favor of the recall over the anticipation method was very small and nonsignificant (*F* less than 1). However, as indicated by a significant third-order interaction of methods with ORM and similarity (*F* = 6.18, df = 2/132, *P* less than .025), this lack of a difference can be largely attributed to the depressed performance of a single group (medium-similarity 50%-ORM recall method) as compared with other groups with corresponding values of any of the three variables. The extent to which this represents a sampling error involving the selection of unusually poor *S*s for this single group, rather than a real phenomenon,

TABLE 12

Mean Correct Responses per Pair on D, F, and N ubsets for Each Group

			Similarity Condition			
Method	**ORM**		**High**	**Medium**	**Low**	**Total**
Recall	100%	D	6.75	16.15	19.02	13.97
		F	8.85	17.25	17.35	14.49
		N	7.06	16.08	18.58	13.91
		Total	7.56	16.49	18.32	14.12
Recall	50%	D	9.04	7.77	15.29	10.70
		F	8.40	7.85	12.81	9.69
		N	8.23	9.10	15.42	10.92
		Total	8.56	8.24	14.51	10.44
Recall	Total	D	7.89	11.96	17.16	12.34
		F	8.63	12.55	15.08	12.09
		N	7.65	12.59	17.00	12.41
		Total	8.06	12.37	16.41	12.28
Anticipation	100%	D	10.17	14.10	16.85	13.71
		F	7.56	12.75	14.94	11.75
		N	8.52	13.21	16.35	12.69
		Total	8.75	13.35	16.05	12.71
Anticipation	50%	D	6.73	14.77	15.54	12.35
		F	6.77	12.83	13.83	11.15
		N	7.35	13.73	14.44	11.84
		Total	6.95	13.78	14.60	11.78
Anticipation	Total	D	8.45	14.44	16.20	[illegible]
		F	7.17	12.79	14.39	11.45
		N	7.94	13.47	15.40	12.27
		Total	7.85	13.57	15.33	12.25
Total	100%	D	8.46	15.12	17.94	13.84
		F	8.21	15.00	16.15	13.12
		N	7.79	14.65	17.47	13.30
		Total	8.15	14.92	17.18	13.42
Total	50%	D	7.88	11.27	15.42	11.52
		F	7.58	10.34	13.32	10.42
		N	7.79	11.42	14.93	11.38
		Total	7.75	11.01	14.56	11.11
Total	Total	D	8.17	13.20	16.68	12.68
		F	7.90	12.67	14.73	11.77
		N	7.79	13.03	16.20	12.34
		Total	7.95	12.97	15.87	12.26

is largely a matter of speculation. In addition to the third-order interaction, a significant methods by ORM interaction was also obtained (F = 4.24, df = 1/132, P less than .05), attributable to the superiority of recall- over anticipation-method performance for the 100% ORM groups while the reverse was true for the 50% ORM groups, which may also reflect at least in part the unusually low performance of the single (medium-similarity 50%-ORM recall-method) group. The direction of this interaction is, however, consistent with the initial hypothesis that if the clear-cut separation of learning from performance measurement characteristic of the recall method is destroyed under the 50% ORM condition, the superiority of recall over anticipation performance is eliminated and perhaps reversed. None of the other interactions involving the three between-group variables approached statistical significance.

Although not included in Table 12, a nonsignificant difference (F = 3.26, df = 1/1479, P less than .10) was obtained in favor of performance on D(R)-pairs (M = 13.09) as compared with D(S)-pairs (M = 12.28), in contrast with the differences in the opposite direction in the first two experiments. Since correct responses on (S) and (R) subsets differed significantly only for the D-pairs of the first experiment, and differed neither consistently nor significantly under any conditions of the other two experiments, it would appear that paired-associate performance is affected little if any by whether "right" or "wrong" syllables from the verbal-discrimination task are used as stimulus- or response-terms in the paired-associate pairs.

There is a remarkably high order of consistency among the three experiments as to the direction and magnitude of transfer effects in paired-associate performance from the various types of pairings of the paired-associate syllables in a prior verbal-discrimination task. None of the experiments show significant facilitation when the pairs are identical in the two tasks, and all show poorer paired-associate performance if the syllables are paired differently in the two tasks, as compared with N-pairs consisting of syllables not used in the verbal-discrimination list. It is therefore apparent that, [illegible] an operational means for producing and isolating familiarization and discrimination processes involved in paired-associate tasks, verbal-discrimination learning primarily involves processes quite different from those required in paired-associate learning, and that despite their close formal and procedural similarity, the two tasks have little in common. However, this does not necessarily imply that neither familiarization nor discrimination processes are important in paired-associate learning, but only that such processes are basically different from those involved in a verbal-discrimination task. This is further indicated by the fact that the transfer effects from verbal-discrimination learning were essentially the same under all conditions of intralist similarity in the last experiment, although this variable must of necessity affect the importance of discrimination processes in the paired-associate task.

Since the predicted differences between paired-associate performance on D- and F-pairs were obtained both in the first and third experiments, but both were depressed relative to performance on N-pairs, the presence of some general interfering factor common to all pairs containing syllables from the verbal-discrimination list would appear to be indicated. One likely source of such an effect, a general confusion resulting from the complex nature of the relationships of the various paired-associate pairs to the verbal-discrimination task, is contraindicated both by the virtually identical findings in the second experiment where a separate group of Ss learned the N-pairs, and by a previous study directly comparing transfer under a within-Ss mixed-lists design strikingly similar to that of the present experiments, with a between-groups design where each S served only under a single transfer condition, the results of which were indistinguishable under the two types of designs (Twedt & Underwood, 1959). That any of the differences between the verbal-discrimination and paired-associate procedures should have interfered generally with both D- and F-pair performance is rendered unlikely by the fact that such procedural differences were identical for the N-pairs and should have influenced them in exactly the same manner. But whatever the ultimate explanation for the various problems raised by the results of these experiments, it is clear that they represent a complete failure with respect to the purpose for which they were originally undertaken, the experimental isolation and operational specification of the familiarization and discrimination processes involved in paired-associate learning tasks.

B. Serial-Position Effects in Paired-Associate Learning (Brown & Battig, 1962).

The serial order in which the items of the list are presented represents a necessary but almost totally ignored factor in paired-associate tasks, principally because the use of unsystematically varied serial orders on successive trials, which has become virtually standard procedure in paired-associate learning experiments (including the present ones) serves to render serial-position cues irrelevant to the learning task required of S, as well as making this factor totally unamenable to systematic experimental investigation. While serial position of items in a list may thus be made irrelevant or inappropriate to the learning required in a paired-associate task, this does not prevent serial cues from still playing an important and significant role in paired-associate learning, albeit primarily as a source of interference with the required learning. Because of the almost complete lack of empirical information concerning the role of factors related to serial position or order in paired-associate learning, and since early evidence suggested that such factors may be of considerably greater performance than has generally been recognized, three separate experiments were carried out under the present project which were primarily directed toward this problem, along with a fourth experiment representing a partial replication of the third. Although the first two to be described were closely related in many respects, and have been published as a single report (Brown & Battig, 1962), they differed sufficiently so that they can most meaningfully be described separately for purposes of this report. The third and fourth experiments, both of which differed considerably from the first two, will be described together in a single subsection.

(1) *Effect of contingent partial serial-position constancy in paired-associate learning*. In this experiment, each of 20 *Ss* learned two lists of ten minimally-similar nonsense-syllable pairs on successive days. The items were presented on 3 x 5 in. cards by the recall method, to a criterion of two successive errorless trials or for a maximum of 10 trials. Each *S* learned one list under the usual *varied-order* procedure, whereby a different serial order is used on each trial, the exact sequence having been predetermined so that no item appeared in the same serial position on more than one trial. The other list for each *S* was learned under a modified *contingent-order* condition, wherein each pair, once it had *first been* responded to correctly, was retained on all subsequent trials in the serial position in which that first correct response was made. Pairs not yet responded to correctly were presented in a different serial position on each trial, as under the varied-order condition. The nature of the contingent-order condition further required that the same serial order be used for the pairs and stimulus-terms alone within each recall-method trial (which was also done for the varied-order condition). The pairs were completely counterbalanced across *Ss* in each condition with respect to serial positions, as a control for possible contamination by differences in pair difficulty. Half of the *Ss* learned their first list under the varied-order condition, and the second under contingent-order, whereas the reverse sequence of the two conditions was employed for the remaining 10 *Ss*. The two lists, on which performance did not differ (*F* less than 1), were also completely counterbalanced with respect both to days and to contingent- or varied-order conditions.

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TABLE 13

Mean Errors per Pair

Error Measure	Varied-Order Easy Pairs	Varied-Order Hard Pairs	Varied-Order Total	Contingent-Order Easy Pairs	Contingent-Order Hard Pairs	Contingent-Order Total
Before First Correct Response	1.78	4.52	3.15	1.66	4.60	3.13
After First Correct Response	0.50	1.40	0.95	0.29	0.60	0.45
Total Errors	2.28	5.92	4.10	1.95	5.20	3.58

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As shown in Table 13 above, significantly fewer total errors were made under the contingent-order condition (*F* = 4.47, df = 1/17, *p* less than .05). However, there was no evidence that this facilitation was due to differential performance in particular serial positions, inasmuch as the contingent-order serial-position curve did not deviate significantly from a straight horizontal line (*F* = 1.72, df = 9/324, *p* approximately .10), nor from the corresponding curve for the varied-order condition (*F* less than 1) by trend analysis of variance.

Instead, as also shown in Table 13, the superiority of the contingent-order condition rested almost exclusively in the reduced number of errors after the first correct response to the five most difficult pairs (defined individually for each S on the basis of total errors per pair). Analysis of variance showed both the overall difference in after-errors between contingent- and varied-order conditions ($F = 9.89$, df = 1/19, P less than .01) and the contingent-varied condition interaction with easy-hard pairs ($F = 7.23$, df = 1/19, P less than .01) to be significant. However, none of the group differences in errors before the first correct response approached significance (all Fs less than 1).

The absence of any kind of serial-position effect under the contingent-order condition, which provides a pure and uncontaminated index of the relative difficulty of paired-associate learning in particular serial positions, came as somewhat of a surprise. However, this finding offers assurance that the use of otherwise highly efficient within-Ss mixed-list designs (see Sections II-A and III-A for examples) is not subject to serious contamination by the differences between conditions in serial position which are an inevitable characteristic of such designs. Even more unexpected was the finding that the effect of holding correct pairs constant in serial position following the first correct response is apparently specific to those pairs and does not facilitate the initial learning of other pairs in the list, as indicated by the absence of any differences between conditions in errors preceding the first correct response. It was largely to determine the generality of this latter finding that the second experiment in this series was undertaken.

(2) Effects of noncontingent partial serial-position constancy in paired-associate learning. Each of 18 Ss learned three lists of 10 nonsense-syllable pairs on three successive days under conditions identical in all important respects to the first experiment, except for the use of noncontingent conditions of partial serial-position constancy in two of the lists learned by each S, whereby either 2 or 4 nonadjacent pairs appeared in the same serial position on all trials while the remaining 6 or 8 pairs were presented in several different serial positions over trials. The third list for each S was identical to the varied-order condition of the previous experiment. Each of the six possible sequences of the three conditions with 0, 2, and 4 constant pairs was used for an equal number (three) of Ss, and each of three lists (on which performance again did not differ (F less than 1)) was used equally often on each day and under each condition. Each serial position was used with about equal frequency for the constant pairs within the 2- and 4-constant conditions, and no S had the same serial position held constant in both his 2- and 4-constant lists.

In contrast with the previous experiment, Table 14 (on the next page) shows that fewest total errors per pair were made under the 0-constant (varied-order) condition, and that number of errors increased progressively for the 2- and 4-constant conditions, an effect shown to be significant by trend analysis of variance ($F = 5.35$, df = 1/24, P less than .05). Similar differences were obtained both in number of errors before and after the first correct response, although these were statistically significant only for after-errors (F = 4.29, P less than .05) but not for before-errors (F = 2.57, P greater than .10), which suggests, in agreement with the first experiment, that any effects of serial-position cues in paired-associate learning are localized primarily in performance after the initial learning of the correct response. Table 14 also shows little difference between performance on constant and varied pairs within the 2- and 4-constant conditions, all of which fell far short of significance (all Fs less than 1). These results all point to the inconsistent or ineffective use of serial-position cues by Ss under the 2- and 4-constant conditions, involving a failure to utilize these cues appropriately in learning constant pairs as well as their inappropriate use for varied pairs, so that overall performance under these conditions was significantly retarded. The fact that Ss continued to make use of serial-position cues under the 2- and 4-constant conditions, even when their performance was significantly interfered with thereby (so that they would have been better off to ignore these cues entirely), offers powerful testimony to the importance of serial-position cues in paired-associate learning tasks in general.

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TABLE 14

Mean Errors per Pair for Constant and Varied Pairs

Error Measure	0-constant	2-constant			4-constant		
	Total	Constant Pairs	Varied Pairs	Total	Constant Pairs	Varied Pairs	Total
Before-Errors	2.13	2.28	2.35	2.34	2.38	2.53	2.46
After-Errors	0.51	0.31	0.58	0.52	0.78	0.70	0.74
Total Errors	[illegible]	[illegible]	[illegible]	[illegible]	3.23	3.23	3.20

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As for the opposite direction of these differences from the facilitating effects of contingent partial serial-position constancy in the first experiment, this can most likely be accounted for in terms of the greater difficulty of identifying and discriminating between constant- and varied-order pairs under the present noncontingent conditions. In the first experiment, constant and varied pairs differed also in having been responded to correctly and incorrectly on previous trials, a discrimination which is relatively easy and obvious for Ss in any paired-associate experiment. Although empirical data are lacking on the question, it is probable that the discrimination between constant and varied pairs would become maximally difficult as the numbers of the two types of pairs approach equality, so that had the proportion of constant pairs increased beyond 50%, a shift to increasingly facilitated performance would be expected. Studies showing that paired-associate performance under complete

100% constant serial order is either superior to (McGeoch & McKinney, 1937; McGeoch & Underwood, 1943: Newman & Saltz, 1962) or no worse than varied-order performance (Martin & Saltz, 1961) are at least partially consistent with this interpretation. However, the discrepancies in the above studies presented sufficient difficulties with respect to the general understanding of the role of serial-position factors in paired-associate learning, so that additional experimentation appeared to be called for in order to investigate possible variables which might account for these differences in results.

(3) Effect of learning method on paired-associate learning under constant- and varied-order conditions. With a single exception (McGeoch & Underwood, 1943), all previous studies comparing paired-associate performance, when the items were presented in exactly the same (constant) order on all trials with the usual varied-order condition, had employed a method of presentation and performance measurement which appeared sufficiently unstable and insensitive to differences between these conditions, that the discrepancies in the results of previous studies might be attributable largely or solely to methodological inadequacies. The procedure typically used in these studies, designated herein as the recall-5 method, was similar to the recall method employed throughout the present project in its complete separation of presentation of pairs for learning and of stimulus-terms alone for attempted response-recall, but differed in that the latter occurred only after the entire list of pairs had been presented five successive times instead of after every single series of pair presentations. In the present experiment, this same recall-5 method was used for one of three groups of 16 Ss each, while recall and anticipation methods identical to those described previously in this report (Section I-B) were employed for the other two groups. Each S in all groups learned the same two equivalent lists of 10 minimally-similar nonsense-syllable pairs, one under varied-order conditions identical to the previous two experiments (except that a different serial order was used on each pair-presentation and recall series under the recall and recall-5 methods), and the other list under a constant-order condition wherein the items were presented in exactly the same order each time through the list. Half of the Ss in each group learned their first list under varied-order and their second under constant-order conditions, whereas the reverse (constant-varied) sequence of conditions was used for the remaining eight Ss within each group. The two lists were also used equally often under each condition and on each of the two days, but the same one of two Es (experimenters) was always used for all Ss learning a given list, so that the two lists were completely confounded with experimenters.* All materials were presented by automatic slide projector at a 4-sec. rate and learned to a criterion of two successive errorless trials or a maximum of ten trials, except under the recall-5

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* This confounding was intentional, because it simplified the task for each E by requiring him to use only one list, and simultaneously permitted the control of two possibly influential variables which were not of direct concern to this experiment.

method, where all Ss received 10 series of pair-presentations, with the stimulus-terms being presented alone (for 20-sec. each) only after the fifth and tenth of these.*

TABLE 15

Mean Total Errors per Pair for Constant- and Varied-Order Conditions for Each Day and Learning Method

Method	Day 1		Day 2		Both Days	
	Constant	Varied	Constant	Varied	Constant	Varied
Anticipation	3.69	6.49	6.79	4.45	5.24	5.47
Recall	5.41	5.26	5.03	5.95	5.22	5.61
Recall-5	3.15	4.45	4.05	4.65	3.60	4.55
Total	4.08	5.40	5.29	5.02	4.69	5.21

Table 15 above presents mean total errors for each group under constant- and varied-order conditions separately for the lists learned on the first and second day as well as for the two days combined. It is apparent from this table, and also from the results of an analysis of variance, that performance on the list learned on the second day was more closely related to whether the first-day list was learned under constant or varied order, than it was to the actual conditions under which the second list itself was learned. In other words, all groups performing under constant conditions on the first day differed very little in their second-day performance under varied-order conditions from the first day, and a similar lack of difference was evident for the remaining *Ss*, who learned a constant-order list on the first day and a varied-order list on the second, particularly as compared with the magnitude of the differences between groups and constant-varied order conditions within any given day. Unfortunately, the present design totally confounds these sequential effects with individual differences between *Ss*, but whatever the relative contribution of these factors, it is clear that the second-day data thus become of limited value and difficult if not impossible to interpret meaningfully. A further indication that performance on the second day was significantly influenced in a negative manner by the first-day condition was the finding of a near-significant overall *inferiority* of second-day to first-day performance by analysis of variance (*F* = 3.66, df = 1/39, *P* less than .10), contrasting with the positive and usually significant effect of practice in previous studies employing this type of design. This analysis, however, showed no significant overall effect of the constant-varied order conditions (*F* = 1.11), nor any interaction of this variable with methods (*F* = 1.61, df = 2/39, *P* greater than .25).*

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* After the conclusion of paired-associate learning on each day, each S learned one serial list consisting of the 10 stimulus-terms from the paired-associate list, and a second consisting of the response-terms, the stimulus-response list order being different on the two days for each S as well as counterbalanced across days, lists, and conditions for the various Ss in each group. Following the constant-order paired-associate list, half of these Ss learned serial lists which were in the same order as the paired-associate lists, whereas the serial lists were in a different order from paired-associate learning for the remaining Ss. Since these results at best are indirectly relevant to the principal issues involving processes in paired-associate learning, they will not be presented or discussed in detail here, but will be included in a planned published report of this and other related experiments currently in progress (Battig, Brown, & Nelson, in preparation). In general, these results showed better serial-list performance on response- than on stimulus-term lists, better performance following constant-order paired-associate learning on serial lists which were in the same order as the paired-associate list, as compared either with a serial-list order differing from the paired-associate serial order, or with serial-learning performance of the varied-order group. The latter findings in particular substantiate the general conclusion that serial-position cues play an important role in paired-associate learning, but that this occurs mainly at relatively late stages of learning (since the serial-learning differences were also found under conditions where paired-associate differences between constant and varied-order conditions were absent).

Although still subject to the possible objection (based on the second-day data) that the various groups may have differed substantially in level of learning ability, first-day performance measures are free of other possible contaminating influences, and without question offer the best available basis for comparisons involving the major variables of this experiment. Moreover, analysis of variance based on first-day performance alone reveals a quite different picture, showing a significant overall difference in favor of constant- over varied-order performance (*F* = 4.91, df = 1/36, *P* less than .05) and a near significant conditions by methods interaction (*F* = 3.14, df = 2/36, *P* less than .10). Further analysis, with the recall-5 method excluded, revealed this latter interaction to be significant (*F* = 7.77, df = 1/24, *P* approximately .01), indicating (as shown by Table 15) that constant was superior to varied-order performance only under the anticipation method and not under the recall method.

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* Evaluation of overall differences between the recall-5 method and the other two methods is complicated by the lack of comparable performance measures for these methods. Number of different pairs responded to correctly over Trials 1-5 and 6-10 were judged to represent the measure for the recall and anticipation methods most closely comparable to the available recall-5 measures. This was shown by analysis of variance to be significantly lower for the recall-5 than for the other methods, which did not differ significantly from each other, either for both days combined or for either day alone (all *Fs* less than 1).

Also significant in the first-day analysis of variance was a third-order interaction of methods and constant-varied conditions with the confounded list-*E* variable (*F* = 4.87, df = 2/36, *p* less than .05), shown by subsequent analysis to reflect almost entirely differences within the recall-5 method, wherein constant-order performance was superior to varied-order for one *E* (and list), whereas the opposite was true for the other *E* and list (*F* = 5.48, df = 1/12, *p* less than .05). This interactive effect involving the list-*E* variable not only supports the previous suggestion as to the deficiencies of the recall-5 method in paired-associate experiments, but also appeared to reflect differences between *Es* rather than lists, since the lists had been carefully equated prior to the experiment and had not differed in performance under either the anticipation or recall methods. Further investigation revealed an inadvertent procedural difference between *Es* in their handling of situations where *S* made more than one response during a single stimulus-term presentation (occurrences of which were largely limited to the recall-5 method where a20-sec. response interval was used as compared with 4-seconds for the other two methods). One *E*, who found constant-order superior to varied-order, counted the last response given in such cases, whereas the other *E* always recorded the first response alone that was made by each *S*. This discrepancy, along with the fact that the single previous study finding no difference between constant- and varied-order conditions (Martin & Saltz, 1961) had also used shorter response-times (1-4 sec.) than corresponding studies which showed significant differences, suggested that lengthening response-time interval might increase the difference in favor of constant- over varied-order performance.

A test of this latter suggestion represented the primary purpose of the fourth and final study concerned with serial-position effects under the present project. Each of 16 *Ss* learned on successive days under the recall method the same two nonsense-syllable lists employed in the previous experiment, half learning both lists under the constant-order condition while the remaining eight *Ss* learned both lists under the varied-order condition. For half of the *Ss* within each condition, the stimulus-terms were presented for 4-sec. each (the same rate as used for pair-presentation for all *Ss*), whereas an 8-sec. stimulus-term presentation rate was used for the remaining four *Ss* within each constancy condition. Each list was used equally often within each subgroup on each day, and the one *E* was not either of those involved in the previous experiment.

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TABLE 16

Mean Total Errors per Pair for Each Condition

Response-Time Interval	Day 1		Day 2		Both Days	
	Constant	Varied	Constant	Varied	Constant	Varied
4-seconds	2.70	5.63	3.40	4.93	3.05	5.28
8-seconds	3.35	5.23	2.55	5.23	2.95	5.22
Total	3.02	5.42	2.98	5.08	3.00	5.25

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As shown in Table 16 at the bottom of the preceding page, performance differed little if at all for the 4- and 8-sec. response-intervals within either the constant- or varied-order condition. Only the overall difference in favor of constant-over varied-order performance, which contrasts with the corresponding finding of no difference under the recall method in the previous experiment, was significant by analysis of variance ($F = 5.06$, df = 1/14, P less than .05), all other F-values in this analysis being less than 1. A possible explanation for the discrepancy from the previous experiment lies in the inadvertent use of a single identical serial-order for all *S*s learning a given constant-order list in the previous experiment, whereas a different randomly determined serial order was used for each constant-order *S* in the present study. Should the particular serial order employed in the previous experiment have been more difficult than average, as is not unlikely in view of the typical rules according to which serial-list orders are determined, poorer performance relative to the varied-order condition would be expected than when several different orders, presumably more representative of the difficulty-level of the varied-order condition, are employed.

Because of the numerous uncertainties and complications raised by these two experiments, a replication of all essential conditions of the first one but with adequate controls for those variables apparently complicating its interpretation, which also involves stimulus- and response-term meaningfulness as an added independent variable, is currently under way as part of another research project. Until this (and perhaps still further studies growing out of it) is complete, any conclusions concerning the effects of constant serial-order in paired-associate learning would be decidedly premature. On the basis of present information, however, it appears that (a) serial-position cues do play an important role in paired-associate learning; (b) this role becomes of maximal importance relatively late in the paired-associate process, and may occur primarily following partial or complete learning of the stimulus-response association, serving thereafter as a means of organizing or overcoming interference resulting from other pairs in the list; and (c) the nature and magnitude of the effect of serial-position factors may be highly sensitive to other procedural and task variables (e.g., learning method, kind of material).

DISCUSSION

Points of discussion specific to a particular individual experiment or series of experiments have for the most part been taken up in connection with the descriptions of these experiments. Consequently, the present section will be limited to issues of a more general nature, and will be organized around four general conclusions to be drawn on the basis of the various lines of research performed under the present project, which are of an argumentative nature or appear to call for further discussion on some other grounds.

I. ON THE BASIS OF THE PRESENT RESULTS, THERE IS NO EVIDENCE THAT EITHER PAIRED-ASSOCIATE LEARNING IN GENERAL OR ANY COMPONENT PROCESS THEREOF TAKES PLACE COMPLETELY IN ALL-OR-NONE FASHION IN A SINGLE TRIAL, EITHER TYPICALLY OR NECESSARILY.

All of the six experiments described in Section II which bear directly or indirectly on this question, as well as one originally conceived for an entirely different purpose (Section III-A, p. 34), indicated that paired-associate learning takes place via gradual increments with repetition, whether under conditions designed to isolate an association-formation process or with materials involving also other nonassociative processes. Only when the Ss had been intentionally instructed and pretrained to concentrate on learning a few pairs of the list on each trial while ignoring the remainder ("part" groups in Section II-C), or the materials were demonstrably highly heterogeneous in difficulty (word-bigram pairs in Section II-D), did the repetition condition fail to significantly surpass the performance under the corresponding nonrepetition condition, and even in these two nonsignificant cases, the differences were in a direction favoring repetition performance.

Although extensive efforts were made in the present experiments to evaluate and take account of the uncontrolled variables and other difficulties inherent in the Rock (1957) paradigm which have in previous studies rendered it inadequate as a means of testing the all-or-none thesis, the impression has been fostered in certain quarters (Estes et al., 1960, 1962; Postman, 1961) that this procedure is by nature inappropriate for the investigation of this problem, and should be dropped in favor of some other procedure free of these defects. More by default than for any other apparent reason, the replacement seems to have become some form of the "miniature experiment" paradigm introduced by Estes (1960), wherein one or two "reinforcement" (that is, pair-presentation) series are intermixed with one or more "test" trials (wherein stimulus-terms are presented alone for attempted response-recall), and all-or-none or gradual learning is determined primarily by means of comparisons of performance on subsequent "test" trials between pairs responded to correctly and incorrectly on initial test trials. Unfortunately, most of the disadvantages inherent in the Rock paradigm, and some additional ones besides, have been offered as criticisms of the Estes "miniature experiment" (e.g., Postman, 1961; Underwood, 1961a), and attempts to eliminate these shortcomings have been if anything less successful, and certainly far less extensive, than similar efforts applied to the Rock paradigm. Estes himself has unwittingly put his finger on perhaps the most serious of the shortcomings unique to his procedure in a recent reply to Underwood's (1961a) criticisms (Estes, 1962), wherein he states that Underwood's postulation of "sufficiently complex combinations of complicating factors" is such as to "require a model including more parameters than the number of degrees of freedom in the data." That the simplicity of the "miniature experiment" as compared with the typical paired-associate procedure, and the consequent lack of sufficient degrees of freedom therein, may instead represent strong arguments against the procedure appears to have been overlooked by Estes. Nevertheless, in view of the known high order of complexity of paired-associate learning and the existence of independent empirical evidence for most if not all of the complicating factors suggested to be involved in the Estes paradigm (e.g., the role of differences in pair-difficulty and learning ability of Ss), this latter conclusion would appear to be the more appropriate one.

Recent but considerably less extensive efforts have been made to test the all-or-none issue in the typical unmodified paired-associate situation, which if feasible clearly has much to recommend it as compared with procedures requiring major modifications from the usual paired-associate conditions, which become necessary both within the Rock and Estes paradigms. It is thus most unfortunate that the contaminating effects of differences in learning ability of Ss and in pair difficulty seem to represent even more serious and insurmountable difficulties which have been almost totally ignored in studies to date working strictly within the typical paired-associate situation (Bower, 1961, 1962). Somewhat paradoxically, Bower's close adherence to the paradigm and procedure characteristic of paired-associate learning experiments has been accompanied by a search for simplification which has caused his tasks to bear little resemblance in equally important respects to those of other paired-associate learning experiments, a criticism which is also applicable to most of the studies following the Estes paradigm and to a few within the Rock paradigm. Steps adopted in order to reduce or eliminate contaminating sources of complexity and to achieve maximal simplification within the paired-associate situation have included such as (a) the use of a small number of highly familiar possible response-alternatives (e.g., single-digit numbers from 1-2 or 1-8); or (b) substantial guessing behavior on the part of S, who may be instructed to make some overt response on each trial, which may be further guaranteed by self-paced temporal presentation of materials in lieu of a controlled presentation rate. In addition to their obvious discrepancies from the materials and procedures employed in the present and most other experiments purportedly concerned with paired-associate learning, these procedures are also suggestive of conditions under which one could probably demonstrate the phenomenon of "zero-trial learning," as through the use of pairs like "dog-cat," "black-white," or boy-girl." That this latter phenomenon would more likely be referred to as a case of complete positive transfer from previous learning represents the crux of the present objection to these "simplified" kinds of tasks and materials. To study the nature of basic processes in paired-associate learning, one obviously must provide for conditions wherein these basic processes necessarily occur rather than having previously taken place or being strongly influenced by other closely-related previously established forms of learning.

The present conclusion that there is no evidence of universal all-or-none learning in paired-associate situations thus seems inescapable,, and while the possibility cannot be ruled out that procedures for the investigation of this problem will ultimately be devised which are superior to the modifications of the Rock paradigm employed herein, this claim cannot be made of the alternative procedures currently in vogue, for the reasons outlined herein.*

II. PAIRED-ASSOCIATE LEARNING TYPICALLY INVOLVES A LARGE NUMBER OF COMPLEXLY INTERACTING TYPES OF LEARNING OR LEARNING PROCESSES, THE PRESENCE AND RELATIVE IMPORTANCE OF WHICH MAY VARY CONSIDERABLY DEPENDING UPON THE KINDS OF LEARNING MATERIALS AND PROCEDURES EMPLOYED.

This statement might be more aptly described as an article of faith or an indication of despair, rather than as a rational conclusion following from the empirical results of research under the present project. Nevertheless, a number of findings from highly divergent sources in the present experiments seem to point unmistakably to such a conclusion.

First and perhaps most important, there are the results of the experiments in Section III-A, which not only suggest that quite different processes may be involved in the superficially similar verbal-discrimination and paired-associate tasks, but also that the terms most commonly applied to component paired-associate processes (e.g., discrimination, familiarization, response-learning) may each represent instead a gross descriptive category including several similar but fundamentally distinguishable processes.

Secondly, both the results of the studies on interpair interference (Section II-E) and serial-position cues (Section III-B) in paired-associate learning suggest that recoding or organizational processes, or other means of overcoming interference produced by the context of other pairs in the list within which any single paired-associate pair must be learned, probably play a major role even under paired-associate conditions designed to minimize such interpair interactive effects, but one which has received little more than occasional lip-service in previous attempts to identify or analyze component paired-associate learning processes. Further supporting this position are some observations made in the various studies employing the shape-number pairs which had been designed so as to minimize or eliminate all previously-determined processes, other than association-formation between stimulus-shape and response-number, and appeared to have been reasonably successful in doing so. However, several _S_s reported learning these pairs by comparing or interrelating them to each other, often in an indirect or far-fetched manner, suggesting that some general categorizing or organizing process, distinguishable from the independent formation of stimulus-response associations within each individual pair, may have played a substantial role in the learning of these materials. While such processes can be distinguished from the association-formation process quite readily at a conceptual level, any separation between them becomes virtually impossible in practice, so that some such organizational processes may constitute as basic and important a defining property of paired-associate learning as does the association-formation process itself.

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* Although complete returns are not yet in, there are interesting indications that the sequence of events involving the Rock paradigm, wherein initial evidence supporting the all-or-none position has shifted in more recent and better-controlled studies to a strong preponderance of evidence contrary to this position, is likely to be repeated for the Estes and Bower paradigms (e.g., Jones, 1962; Postman, 1961; Seidel, 1961; Williams, in press).

Even should the underlying nature of all these various component processes of paired-associate learning turn out to be identical, a prospect which seems both unlikely and indeterminate at the present level of conceptualization and analysis, their particular mode of operation or manifestation is likely to vary markedly from one individual _S_, task, or procedure, to another, so that at an empirical level the distinctions will be no less important than if the underlying processes turn out to be basically different. Particularly demonstrative of this point is the type and level of analysis suggested by the selective-mediation model of paired-associate learning offered by Newman (1961), which in addition to encompassing several different processes also deals with a number of distinguishable means by which these necessary processes may be manifested in actual paired-associate tasks and situations. Whatever the number and scope of the complications produced thereby, the present results suggest such an approach to be both appropriate and necessary. It is apparent that future researchers will have little choice but to shift to some such more complex level of analysis, and to devote far more attention to the identification and operational specification of component processes of paired-associate learning than has thus far been the case. It is equally obvious that, until these various component processes have been more adequately analyzed and identified, little is to be gained by further pursuit of problems such as the all-or-none vs. incremental issue which has been basic to the present project.

III. THE ANALYSIS AND UNDERSTANDING OF PAIRED-ASSOCIATE LEARNING IS MORE LIKELY TO BE FURTHER RETARDED RATHER THAN ADVANCED BY CONTINUED ADHERENCE TO BASIC CONCEPTS AND PRINCIPLES DERIVED FROM STUDIES DIRECTLY CONCERNED WITH SIMPLER KINDS OF LEARNING SITUATIONS.

Although this conclusion would seem to be an inevitable consequence of the immediately preceding one, especially convincing evidence in its support is provided by the experiments (Section

I-B and I-C) demonstrating wide discrepancies between the effect of the knowledge-of-results variable in paired-associate tasks, and the central role played by immediate reinforcement in simpler operant-conditioning situations, although these two factors have often been treated as though they were one and the same (e.g., Estes, 1960; Skinner, 1957, 1958). In direct contrast with the operant-conditioning results, which would imply that immediate knowledge of results is essential to efficient learning, the present studies indicate instead that complete omission of the aspect of knowledge of results (right-wrong information) most directly comparable with operant reinforcement procedures has no significant overall effect on paired-associate performance, and that the conditions of delayed knowledge of results provided by the recall method typically lead to performance which is at least equivalent if not significantly superior to that obtained under the immediate knowledge-of-results conditions provided by the anticipation method.

The close dependence of recent programmed-learning and teaching-machine techniques on Skinner's operant-conditioning principles, despite the fact that the procedures and materials involved therein bear far more resemblance to the typical paired-associate paradigm than they do to any of the operant paradigms from which the principles were derived, therefore would appear to be decidedly inappropriate and productive of great potential confusion and harm with respect to the future of this particular applied technological development. While concern with the development of maximally efficient programmed-learning techniques is not a legitimate part of the present project, the fact that significant findings directly relevant to this applied problem have emerged, even from research directed toward quite different non-applied goals, offers the best possible answer to those programmed-learning enthusiasts who have accepted almost as an article of faith the proposition that "the science of human learning has been largely irrelevant to both educational engineering and to theories of learning" (Gilbert, 1960).

With regard to programmed-learning techniques, the key suggestion from the present results is a very simple one which is not at all inconsistent with "common sense." Stated explicitly, it implies that interrupting a continuing performance repeatedly by some sort of interpolated knowledge of results is a less generally efficient procedure for learning than presenting this knowledge of results only after the conclusion of complete units or at natural "breaks" in the sequence. That the basic principles underlying programmed-learning techniques appear already to have become so rigidly fixed, even in the early infancy of this development, to lead workers in this area to summarily reject such findings as these as irrelevant or immaterial, rather than subjecting their implications to empirical test, is indeed a sad commentary on the extent to which this area has come under the control of the narrow-minded pseudo-scientist and the economic profit-seeker.

The preceding discussion of the inapplicability of simple reinforcement principles to paired-associate learning is intended here principally as a particularly vivid illustration of the more general difficulties created by the approach of treating paired-associate learning as little beyond a multiple-response form of simple conditioning, to be described and accounted for in terms of principles derived from simpler learning studies. With few exceptions (e.g., Miller, Galanter, & Pribram, 1960), investigators in paired-associate learning have not departed radically from the stimulus-response type of approach which has dominated conditioning research, and have also made extensive use of many of the concepts (e.g., reinforcement, extinction, habit-family-heirarchy, etc.) originally formulated in simpler-learning contexts. This resemblance can be easily seen even in the most complex current theoretical models of paired-associate learning (e.g., Newman, 1961). The present position is not that these various types of learning do not have certain communalities with respect to basic phenomena and principles, but merely that nothing but trouble and misunderstanding is likely to be produced by beginning with the assumption that complex (e.g., paired-associate) learning research is to be approached as though this were necessarily the case. All available evidence points to the probable inadequacy of such an approach, since not only are the critical variables in simple learning (e.g., reinforcement) shown to be of little importance in paired-associate situations, but those variables which are of demonstrable significance in paired-associate learning, such as meaningfulness (Underwood & Schulz, 1960), often have no meaning whatsoever in simpler conditioning situations. While it runs counter to usual scientific practice, a more reasonable alternative strategy would appear to be to approach paired-associate learning as though it were a separate and independent phenomenon, and to devise procedures and hypotheses with minimal attention to possible similarities or relationships to simple types of learning. Should the empirical results of such studies point to basic communalities with simpler learning situations, this will soon become self-evident, and common concepts or principles demonstrated in this manner will be far more significant and meaningful than the kind of pseudo-communalities emanating from the original selection of experimental variables, tasks, or procedures on the basis of their correspondence with simpler learning situations, which still predominates in current paired-associate learning research. To the extent that fundamental differences in fact exist between the two types of learning situations, an approach based on the assumption of independence between them is obviously more likely to be successful in the discovery and evaluation of these differences.

IV. IN ORDER TO EVALUATE AND TAKE INTO ACCOUNT THE NUMEROUS DEMONSTRATED OR SUGGESTED INTERACTIONS BETWEEN VARIABLES AS TO THEIR EFFECTS ON PAIRED-ASSOCIATE LEARNING, EXTENSIVE USE OF COMPLEX MULTI-FACTOR EXPERIMENTAL DESIGNS IS BECOMING INCREASINGLY ESSENTIAL FOR RESEARCH IN THIS AREA.

This statement also follows almost as a direct corollary of the preceding two conclusions and the arguments offered in their behalf, but is subject to somewhat more direct verification based on actual empirical evidence obtained in the present project. Of particular significance in this respect are the findings of those experiments described in Section III, indicating that the effects of other variables (e.g., previous verbal-discrimination learning, constant- or varied-serial-order, percentage occurrence of response-members) may be quite different under the recall as compared with the anticipation method, and the further suggestion that either different processes or differential emphases on the same processes may be involved under these two methods. Although less convincing at its current limited stage of progress, a similar widespread interactive influence with other variables may occur with respect to the use of varied as compared with partially or completely constant serial order of presentation of the paired-associate list. The various experiments described in Section II are also replete with evidence for interacting variables, showing that relative performance under repetition and nonrepetition conditions may vary considerably depending on method of learning or the kind of learning material employed. Although not extensively investigated as a systematic variable, this latter factor appears to be generally prone to interacting influences in the present studies (e.g., recall as compared with anticipation method) as well as other more extensive research (Underwood & Schulz, 1960). Under such conditions, any approach other than a complex multifactor design incorporating a maximum number of independent variables would appear to be minimally appropriate, inefficient, and a likely source of apparent inconsistencies or conflicts between results of different experiments concerned with similar problems, a retarding influence which has already been all too common in paired-associate experimentation and has done much to bring on the not entirely unjustified charge that paired-associate research has been concerned largely with trivial pseudoproblems. Fortunately, the major drawback to the use of multifactor experimental designs, the extensive time and effort they require, has been rendered largely obsolete by the development of the digital computer. Of course, there remains the problem of overcoming the widespread resistance against the use of complex procedures or the investigation of complex psychological phenomena, but there is reason to hope that the resulting partial abandonment of the increasingly complex (but definitely not sinking) paired-associate ship may be at least partially counteracted by the fact that there is obviously much more to be done in this area than previously appeared to be the case, and that researchers still tend to be attracted by currently active and controversial problem areas.

In this latter connection, due to the failure of the Cooperative Research Branch of the Office of Education to recommend approval of support for a new research proposal designed to permit continued investigation of the problems raised by the research described in this report, it is to be hoped that this negative action does not reflect a general policy with regard to the type of research described herein. Thanks to approval of an amendment providing a year's extension of the present contract with the Cooperative Research Branch, and the subsequent acceptance of the research proposal for support by other Federal agencies, it has been possible to carry on with the general research program, begun and extensively developed under the present project, without serious interruption. Nevertheless, it would be most unfortunate if the necessity and potential significance (applied as well as pure) of a considerable expansion of the kind of research attempted under the present project were not to be sufficiently recognized by all those concerned with any aspect of human learning, and it is hoped that the present report has been successful in conveying its message as to the importance of maximal encouragement and support for basic laboratory research devoted to the study of human learning. In addition to the more obvious and direct necessity for such an approach from a theoretical point of view, it is equally essential as a prerequisite to the success of more applied and technological developments concerned with the improvement of learning efficiency.

SUMMARY

The principal purposes of this project were to investigate and analyze the roles played by various component learning processes involved in the typical paired-associate learning situation, wherein each of several different stimulus-terms are to be associated with and responded to by a unique one of a large class of possible alternative responses. Special emphasis was directed toward the isolation of an "association-formation" process generally regarded as primary in paired-associate learning, in order to determine whether either this process or paired-associate learning in general necessarily takes place in all-or-none fashion in a single trial, rather than building up in strength in a gradual incremental manner with repetition in line with the traditional conception. The variety of experiments conducted in the pursuit of one or more of these goals are summarized under three major headings: (I) Development of materials and procedures adequate for the proposed research; (II) Studies of the role of repetition in paired-associate learning and the component association-formation process thereof; (III) Studies of other processes in paired-associate learning.

I. Development of materials and procedures.

A. Development of learning materials. Extensive preliminary efforts were devoted to the construction and calibration of pairs consisting of nonsense-shapes as stimuli and two-digit numbers as responses, designed to be maximally homogeneous in learning difficulty, and to require a relatively substantial amount of the association-formation process of paired-associate learning while maximally free of the influence of other known component processes thereof. Previously calibrated homogeneous nonsense-syllable pairs were also employed extensively, with occasional use also of various other types of learning materials.

B. Comparison of recall and anticipation methods. Since the recall method required in most of the present research, wherein each trial begins with the presentation of the entire list of pairs followed by the presentation of all stimulus-terms alone for attempted response-recall, differed considerably from the anticipation method characteristic of most paired-associate studies, an early experiment provided for a direct comparison between the two methods. These results showed a sizable difference in performance in favor of the recall over the anticipation method, despite the immediate and complete knowledge of results provided under the latter procedure and its close correspondence to the procedures currently in vogue in programmed-learning techniques. Two subsequent experiments also including this variable showed that the differences in favor of the recall method tended to be reduced or eliminated under other conditions, but further indicated that the effect of other variables on paired-associate performance differed considerably depending on which of the two methods was used, and suggested that somewhat different learning processes may be involved under the two methods.

C. *Knowledge of results*. In support of the implications of the previous studies regarding the probable unimportance of immediate knowledge of results as a necessary condition for efficient paired-associate learning, it was shown that presence or absence of immediate knowledge of results (right or wrong) following each attempted response under the recall method had no significant overall effect on performance.

II. *Studies of the role of repetition in paired-associate learning*.

A. *Learning of shape-number pairs under simultaneous repetition and nonrepetition conditions*. Lists of 12 of the shape-number pairs were arbitrarily divided into two equivalent subsets of six each, differing in that the *repetition* subset was presented on all trials, whereas pairs of the *nonrepetition* subset were presented on succeeding trials only if correct the first time they were presented, otherwise they were eliminated and replaced on the next trial by new previously unseen pairs. Contrary to an all-or-none theory and to previous results of Rock and others showing no difference between different groups of *Ss* under repetition and nonrepetition conditions, performance proved to be significantly better on the repetition than on the nonrepetition subset.

B. *Role of repetition in paired-associate learning with all correct (learned) pairs eliminated*. Except for the elimination and replacement by new pairs of *all* correct pairs after each trial within both the repetition and nonrepetition subsets, the materials and procedure were identical in this experiment to the previous one. The elimination of correct pairs vastly increased the sensitivity of the procedure to the detection of differences in favor of the repetition over the nonrepetition subset, and further analysis of pairs of the repetition subset revealed a significant increase in proportion of correct responses as number of previous incorrect presentations increased.

C. *Effect of method of pretraining and knowledge of results on repetition and nonrepetition performance*. Different groups of *Ss* learned under repetition and nonrepetition conditions corresponding to those described in (A) above, lists of nonsense-syllable pairs representing all possible combinations of presence or absence of knowledge of results, and of three pretraining conditions. The latter consisted of *no* pretraining, *part* pretraining (wherein *Ss* first learned each of six lists to a criterion of 4 out of 12 correct pairs and were instructed to learn by concentrating on a few pairs on each trial while ignoring the remainder of the list), and *whole* pretraining (under which each *S* had previously learned two lists to one errorless trial and was instructed to try to learn equally all pairs on each trial). Knowledge of results had no effect on either repetition or nonrepetition performance. Contrary to Rock's results and in agreement with the previous two studies, a significant superiority of repetition over nonrepetition performance was found following whole or no pretraining. However, the nonrepetition group given part pretraining performed at a level comparable to all of the repetition groups, indicating that apparent one-trial all-or-none learning can be produced if Ss inappropriately concentrate on learning only part of the list of pairs.

D. Effect of kind of material and previous experience on repetition and nonrepetition performance. Four very different lists (pairs of nonsense-syllables, adjectives, words-bigrams, and shapes-numbers) were learned by each S under various combinations of repetition and nonrepetition conditions like those described in (C). Significantly superior repetition performance of approximately equal magnitude was obtained for all types of materials except the word-bigram pairs, which was attributed to the fact that these were the only materials not previously equated for learning difficulty and were found to be demonstrably heterogeneous in this respect. Neither repetition nor nonrepetition performance was affected in any way by previous experience under nonrepetition as compared with repetition conditions, possibly because many of the lists were so easily learned that little distinction in practice existed between the two types of procedures.

E. Differences in interpair interference under repetition and nonrepetition conditions. Four groups learned different lists consisting of various combinations of learned and unlearned pairs selected from a previous list learned to a criterion of 6 out of 12 correct responses, with new previously unseen pairs. The lists for the four groups included learned-unlearned, learned-new, unlearned-new, and new-new combinations of pairs, respectively. Learned pairs were found to interfere less with the learning of other pairs than did either unlearned or new pairs, which did not differ significantly from each other in their interference with other pairs. However, despite the demonstrably greater difficulty of unlearned as compared with new pairs, performance on them on early trials was significantly superior to new pairs, providing further evidence in opposition to the all-or-none learning thesis. A subsequent exploratory study, designed to eliminate deficiencies in the previous procedure as a means of determining the precise effects of number of previous repetitions on interference with the learning of other pairs, proved to be largely unsuccessful, and provided no support for the original hypothesis that more interference from unlearned than from new pairs may account for previous failures of repetition performance to exceed that under nonrepetition conditions.

III. Studies of other processes in paired-associate learning.

A. Transfer from verbal-discrimination to paired-associate learning. Three closely similar experiments dealt with this problem, the verbal-discrimination task originally being viewed as a possible means of operationally specifying and separately evaluating the role played by discrimination and familiarization processes in paired-associate learning. However, none of the three experiments showed any evidence for positive transfer from verbal-discrimination learning (wherein S learns merely to repeat the arbitrarily-designated correct nonsense syllable of each pair when that pair is presented) to paired-associate learning, even when the syllables were paired identically on the two tasks. Moreover, negative transfer effects were obtained in all experiments when the syllables had previously been paired differently on the verbal-discrimination task, these effects being of equal magnitude whether the verbal-discrimination pairings were with syllables included in or excluded from the paired-associate list. This negative transfer proved to be primarily attributable to pairs not involving measurable intrapair associations produced by verbal-discrimination learning, offering indirect evidence of gradual as opposed to all-or-none learning and suggesting further that maximal interference may be produced by partially-learned pairs. The final experiment, investigating the effects of intralist similarity, recall vs. anticipation methods, and percentage occurrence of response-members (50% or 100%) on these transfer phenomena showed that the magnitude of the transfer effects was attenuated under the recall as compared with the anticipation method. Although the other two variables both had significant effects on overall paired-associate performance, neither affected significantly the type of transfer obtained from verbal-discrimination learning. These results suggest that quite different processes are involved in verbal-discrimination as compared with paired-associate learning tasks, and that the former procedure does not provide a useful means of operationally identifying and specifying component paired-associate learning processes.

B. Serial-position effects in paired-associate learning. Four experiments were concerned with the role played by the serial-position cues which are inevitably present in paired-associate tasks, but are usually made irrelevant by unsystematically varying serial order from trial to trial. The first experiment showed that, as compared with the usual varied-order condition, paired-associate performance is significantly facilitated if only the correct pairs are maintained on subsequent trials in the serial position in which they were first responded to correctly. This facilitation, however, was almost entirely attributable to a reduction in errors after the first correct response on the last-learned pairs, and no evidence was found of differential performance in particular serial positions under this condition. A second experiment showed that, if 2 or 4 nonadjacent pairs of a 10-pair list are held constant in serial position on all trials while the remaining pairs are varied in this respect, performance becomes significantly and increasingly worse than under a complete varied-order condition. The third experiment revealed 100% constant serial order to be significantly superior to varied-order only under the anticipation method and not under the recall method, with inconclusive results under a third method wherein five pair-presentation series preceded each recall series. The final experiment showed no effect of response-time interval on comparative constant and varied-order performance, but in conflict with the previous experiment found constant- to be significantly superior to varied-order. This discrepancy from the third experiment was explained in terms of the use therein of a single identical serial order for all constant-order Ss which may have been of above-average difficulty. While several questions remain to be answered by further research about the role of serial-position factors in paired-associate learning, it is apparent that, despite the usual efforts to make them irrelevant, they still play an important role particularly at relatively late stages of paired-associate learning.

Conclusions

Four general conclusions reached and discussed on the basis of the overall results obtained under the present project are the following.

I. On the basis of the present results, there is no evidence that either paired-associate learning in general or any component process thereof takes place completely in all-or-none fashion in a single trial, either typically or necessarily.

II. Paired-associate learning typically involves a large number of complexly interacting types of learning or learning processes, the presence and relative importance of which may vary considerably depending upon the kinds of learning materials and procedures employed.

III. The analysis and understanding of paired-associate learning is more likely to be further retarded rather than advanced by continued adherence to basic concepts and principles derived from studies directly concerned with simpler kinds of learning situations.

IV. In order to evaluate and take into account the numerous demonstrated or suggested interactions between variables as to their effects on paired-associate learning, extensive use of complex multi-factor experimental designs is becoming increasingly essential for research in this area.

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APPENDIX

List of Papers Read at Professional Meetings

1. Battig, W. F. Association value of nonsense shapes as a function of complexity and curvature. Amer. Psychol. Assoc., Chicago, Sept., 1960.
2. Battig, W. F., & Brackett, H. R. A comparison of anticipation and recall methods in paired-associate learning. Eastern Psychol. Assoc., Philadelphia, April, 1961.
3. Battig, W. F., & Brown, S. C. Partial serial-position constancy in nonserial paired-associate learning. Midwest. Psychol. Assoc., Chicago, May, 1961.
4. Battig, W. F. Transfer from verbal-discrimination to paired-associate learning. Psychonomic Soc., New York, Sept., 1961.
5. Battig, W. F. Repetition and paired-associate learning. A repetition. Eastern Psychol. Assoc., Atlantic City, N. J., April, 1962.
6. Battig, W. F., Brown, S. C., & Nelson, D. Constant vs. varied serial order in paired-associate learning. Midwest. Psychol. Assoc., Chicago, May, 1962.
7. Brackett, H. R. Method of pretraining and knowledge of results in paired-associate learning under repetition and nonrepetition conditions. Virginia Academy of Science, Norfolk, Va., May, 1962. (also Master's thesis)
8. Brown, S. C., & Battig, W. F. Interpair interference in paired-associate learning. Amer. Psychol. Assoc., New York, Sept., 1961. (also unpublished Master's thesis)

List of Personnel

Principal Investigator.

Battig, William F. (Oct. 1959 - June 1962).

Graduate Assistants.

Brackett, H. Ray (Oct. 1959 - June 1962, full-time summers 1960-61).
Brown, Sam C. (Full-time summers 1960-61).
Schild, Mary E. (Sept. 1961 - June 1962).

Undergraduate Assistants.

Nelson, Douglas (April 1961 - June 1962, full-time summer 1961).
Williams, John G. (February-June 1960).
Williams, John M. (Full-time summer 1960).

DEPARTMENT OF
HEALTH, EDUCATION, AND WELFARE
Office of Education
Cooperative Research Branch

Project Description

Analysis of Processes in Paired Associate Learning.

Problem.--This study seeks to determine whether or not associations are ever formed in an all-or-none fashion rather than by a process of gradual repetition.

Major objective.--The objectives are (1) to define and isolate the basic processes involved in paired-associate learning; (2) to specify the important variables related to these processes and the conditions under which each becomes important; and (3) to define and assess the importance of the assotiation formation process in paired-associate learning tasks.

Procedures and design.--The ~~research~~ program ~~will~~ involved three ~~consecutive~~ phases. In the first phase a paired-associate learning task ~~will be~~ was developed in which the "association formation" process ~~has~~ had been isolated. In Phase II the Rock paradigm as modified by the investigator ~~will be~~ was used to investigate the role of repetition in learning the paired-associate task or tasks which ~~have~~ had been developed in Phase I. A list consisting of two sets of stimulus-response pairs ~~will be~~ was used. All pairs of one set, the control set, ~~will be~~ were retained in the list on all learning trials. Pairs of the experimental set ~~will be~~ were retained in the list only if the correct response ~~is~~ was given, otherwise they ~~will be~~ were replaced in the list by new pairs which ~~have~~ had not previously been seen. 118 If, as the investigator anticipates, the results of Phase II indicate that association formationn is not an all-or-none process, an attempt will be made to determine if there are any conditions under which it is sufficiently all-or-none so that repetition is relatively unimportant.

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Project No. 730
Duration: 2 years
September 1959 to August 1961